Infinite Energy™
Cold Fusion and New Energy Technology
Volume 4, Issue 24 • 1999

Cold Fusion Celebrates

10 Years of Revolutionary Science and Technology!

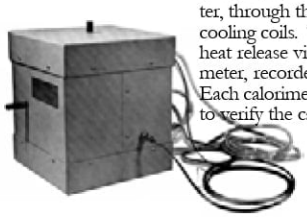
$5.95 U.S.
$7.95 Canada
£3.95 U.K.

MIT and Cold Fusion – A Special Report!

"LIBERTARIANS have all the good ideas."
—HUGH DOWNS, ABC NEWS

For Libertarian Party information, call toll-free or visit our web site.
800-ELECT-US ★ www.LP.org

Seebeck Envelope
CALORIMETER
measures total heat output of cold fusion electrochemical reactions.



Heat flows from the reaction vessel through air or water layers in the calorimeter, through the Seebeck Envelope to an aluminum jacket incorporating water cooling coils. The millivolt output signal from the SEC, which is related to the heat release via an accurate calibration process, can be read out by a millivoltmeter, recorded continuously by any millivolt recorder or fed to a computer. Each calorimeter is supplied with a calibration heater that allows the researcher to verify the calorimeter at any time.

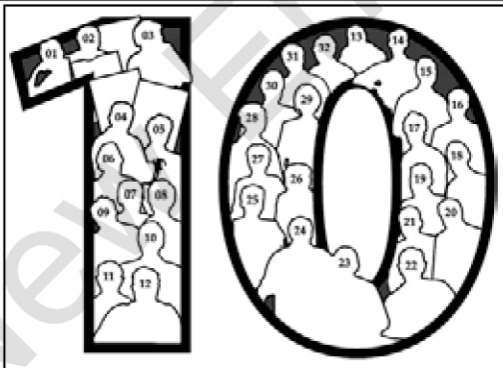
Thermonetics Corporation
7834 Esterel Drive, LaJolla CA 92037
Tel: (619) 453-5483

NEW WATERPOWER TECHNOLOGY FOR THE 21st CENTURY IS AVAILABLE NOW!

Water placed in steel pipe containers store large amounts of elastic compression energy. With ***SUPER FAST JET VALVES,*** invented, made and tested in Pilot Plant conditions by International Molecular Reactor Power Co., Inc., the stored energy is discharged in the form of High Velocity Jets driving a special impulse turbine and electric generator. The velocity of the intermittent jets, developed by the fall *and* expansion of the water compressed by its own weight and water hammer effect, imparts kinetic energy to the turbine capable of generating considerably more power, by far, than the power required to pump back-up the water at "mgh" height. The Molecular Reactor also lets gravity *renew* the stored energy in fractions of a second, between two jets. **This is the most economical, safest and cleanest *RENEWABLE energy* known to man.**

If you are interested in participating in the construction of the Pilot Plant as one of the 50 charter partners or to share the benefits of the most advanced clean power generation technology, please contact: **Nicolas Pataceano, President, International Molecular Reactor Technology Co., Inc.; Tel: (705)431-1940, Fax: (705)431-7849, E-mail: nicpat@bconnex.net; Box 3674, Lilac Dr. SS3, Stroud, Ontario, Canada L0L 2M0**

▼▲▼ ▼▲▼



Cover Photo:
This issue celebrates ten years of revolutionary advances in the science and technology of cold fusion. The individuals depicted on the cover have all contributed in significant ways to the advancement of research, theory, technical innovation and public awareness of cold fusion. We were not able to include many others whose contributions have been equally important to the field simply due to lack of space. Our deep appreciation goes out to all of these talented people as we celebrate an important milestone.

Pictured: Prof. Julian Schwinger (01), Prof. John O'M. Bockris (02), Dr. Eugene Mallove (03), Dr. James A. Patterson (04), Dr. Irina Savvatimova (05), Jed Rothwell (06), Chris Tinsley (07), Prof. Akito Takahashi (08), Prof. Louis Smullin (09), Dr. Francesco Celani (10), Mike Carrell (11), Dr. Les Case (12), Dr. Mitchell Swartz (13), Dr. Dennis Cravens (14), Prof. Peter Hagelstein (15), Dr. Scott Chubb (16), Roger Stringham (17), Dr. Edmund Storms (18), Dr. Melvin Miles (19), Prof. Robert T. Bush (20), Dr. Mahadeva Srinivasan (21), Sir Arthur C. Clarke (22), Dr. Martin Fleischmann (23), Dr. Stanley Pons (24), Dr. Tadahiko Mizuno (25), Hal Fox (26), Dr. Joseph L. McKibben (27), Prof. Xing Zhong Li (28), Dr. Carol Storms (29), Dr. Michael McKubre (30), Prof. George H. Miley (31), Dr. Peter Glück (32). *Artwork: Jeremy Slayton*

Subscribe to *Explore! for the Professional* to acquire the latest research of

T.E. Bearden
P. Tewari
Robert Beck
G. Belovari

and other cutting edge researchers interested in Alternative Energy and Energy Medicine.

Contact us at:

Explore Publications, Inc.
Post Office Box 1508
Mt. Vernon, WA 98273

sample issue $7.50 (US)

back issues ARE available

credit card orders accepted at 1-800-845-7866 or

visit our website at:
www.explorepub.com

New Energy Times Archive

Infinite Energy™
Cold Fusion and New Energy Technology

Table of Contents - Issue 24, March/April 1999

FEATURES

7 Cold Fusion: The First Ten Years
Commentary *by Scott Chubb, Michael McKubre, George Miley, Keith Johnson, Francesco Celani, Dennis Cravens, John Dash, Peter Glück, Bart Simon, Akira Kawasaki, Harold Aspden, Hal Fox, Mahadeva Srinivasan, Robert Bass, Edmund Storms, Tadayoshi Ohmori, Richard Oriani, John Bockris, Fred Jaeger*
23 Comments on the Tenth Anniversary Contributions *by Jed Rothwell*
29 Key Experiments that Substantiate Cold Fusion Phenomena
35 Device and Process Testing Update
36 BlackLight Power Breakthrough *by Mike Carrell*
58 AChronology of Cold Fusion

TECHNICAL ARTICLES

25 Nuclear Reactions in the Pd/D System: The Pre-History and History of Our Early Research *by Martin Fleischmann*
42 My Life With Cold Fusion as a Reluctant Mistress *by Edmund Storms*
49 Theoretical Indications of the Possibility of Nuclear Reactions at Low Energy *by E. Conte*
55 Experimental Evidence for the Cold Fusion of Protons and Electrons into Neutrons *by E. Conte and M. Pieralice*

MIT SPECIAL REPORT

64 Why "MIT and Cold Fusion"?
MIT and Cold Fusion: A Special Report *by Eugene Mallove*
Partial Chronology of MIT Events Relating to Cold Fusion
81 MIT Lecture by Nobel Laureate Julian Schwinger
84 Exhibits for MIT Special Report
93 "Science Writer Quits MIT News Office" —Reprint of *Lee Edson's* article in National Association of Science Writers (NASW) Newsletter

DEPARTMENTS

3 Breaking Through—Editorial *by Eugene Mallove*
5 Letters to the Editor
56 Letter to the Editor *by R. Santilli;* Response to Letter *by E. Conte*
40 Cold Fusion Comix
41 Briefs
121 Calendar
122 Classifieds
128 *Infinite Energy* Subscription and Catalog Order Form

Cover art: See page 1

Volume 4, No. 24
March 1999 - April 1999

Editor-in-Chief and Publisher
Eugene F. Mallove, Sc.D.

Managing Editor
Barbara A.F. DelloRusso

Associate Editor
Hal Fox
(Fusion Information Center, Salt Lake City, Utah)

Contributing Editors
Jed Rothwell (Georgia)
770-451-9890
Susan Seddon (U.K.)
e-mail: sseddon@infinite-energy.com

Information Systems Manager
Jeremy T. Slayton

Publishing Assistant
Christy L. Frazier

Scientific Advisory Board
Dr. Harold Aspden
Southampton, UK
Dr. Robert W. Bass
Pahrump, Nevada
Dr. John O'M. Bockris
Texas A&M University (ret.)
Dr. Dennis Cravens
Cloudcroft, New Mexico
Dr. Peter Glück
Institute of Isotopic and Molecular Technology, Romania
Dr. Xing Zhong Li
Tsinghua University, Beijing, China
Dr. M. Srinivasan
Bhabha Atomic Research Center, India
Dr. Edmund Storms
Los Alamos National Laboratory (ret.)

Infinite Energy Magazine (ISSN 1081-6372) is published six times a year by Cold Fusion Technology, Inc., P.O. Box 2816, Concord, New Hampshire 03302-2816. Subscription price: $29.95 U.S. and Canada, $49.95 other foreign. POSTMASTER: Send address changes to INFINITE ENERGY, P.O. Box 2816, Concord, NH 03302-2816.

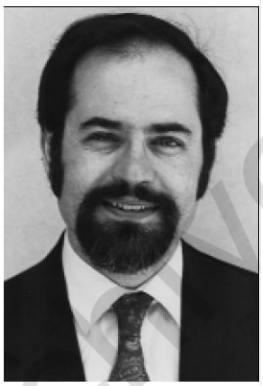
Infinite Energy Magazine
P.O. Box 2816, Concord, NH 03302-2816
Phone: (603) 228-4516
Fax: (603) 224-5975
INTERNET: staff@infinite-energy.com
editor@infinite-energy.com
http://www.infinite-energy.com

Printed in the United States.
Copyright © 1999
Cold Fusion Technology, Inc.
All rights reserved.

New Energy Times Archive

By Eugene F. Mallove, Sc.D.

Ten Years That Shook Physics



When I read George Gamow's book, *Thirty Years that Shook Physics: The Story of Quantum Theory* (Doubleday & Company, 1966), it was impossible to imagine that in less than twenty-five years another revolution would shake physics in ways every bit as dramatic as what happened from 1900 to 1930.

For the past decade, the Cold Fusion and Low Energy Nuclear Reactions revolution has been underway, whether or not the mainstream physics/chemistry establishment and the general science media wish to acknowledge it. This month we celebrate what has indeed been "Ten Years That Shook Physics." The barrier that separated conventionally understood chemistry and nuclear physics has come crashing down like the infamous Berlin Wall. The barrier does not exist—at least not within special microphysical domains of palladium, nickel, and other metals in contact with hydrogen. Exotic new physics is at work, the "End of Science" again disproved.

The Revolution does not even have a *name* on which all the revolutionaries can agree! "Cold Fusion" is likely to stick, if for no other reason than that is where it all began. The terms LENR (Low-Energy Nuclear Reactions) and CANR (Chemically-Assisted Nuclear Reactions) have been tried. This displeases Dr. Randell Mills of BlackLight Power Corporation, who has a radically different theoretical approach and an apparently robust commercial activity. See Mike Carrell's assessment of the new Mills scientific initiative: "hydrino hydride compounds," page 36 - 39. To be neutral on this issue, we'll float a trial balloon. How about "Nu-chemistry?" It's certainly "new" (nu) and it certainly has nuclear (nuc) aspects—as even Dr. Mills agrees.

The Nu-chemistry Revolution began inauspiciously on March 23, 1989 at the University of Utah. Electrochemists Drs. Martin Fleischmann and Stanley Pons had worked for five years and spent some $100,000 of their own funds before they announced their findings. We are very privileged to have in this issue an essay by Dr. Fleischmann, which reveals some of the scientific thinking that led to the discovery (pp.25-28). Circumstances forced disclosure at a press conference some eighteen months before the scientists had wanted to publish.

Fleischmann and Pons claimed in 1989 that in a heavy water electrochemical cell near room temperature they had produced excess energy orders of magnitude beyond explanation by chemistry, and that they had detected neutrons and tritium as well. These were all signatures of nuclear reactions.

Unfortunately, they did not emphasize the difficulty of producing the effects. At the time, because their hands were tied by lawyers focused on patent issues and conflicts with nearby BYU, they were not even able to provide at their news conference a preprint of their forthcoming *Journal of Electroanalytical Chemistry* paper. It was published April 10, 1989 but was circulating via fax almost immediately. A great fuss has been made about this. In retrospect the delay seems short. Tokamak results have often been announced on the 7:00 o'clock news months before papers are made available. Their neutron measurements were flawed, as they later admitted. This was a failing, yet others would later confirm in cold fusion experiments *both* low-level neutron radiation as well as tritium evolution.

Most important to an understanding of the heated debate of the past decade: The Fleischmann-Pons announcement threatened an entrenched Federal research program. Billions of dollars had been invested by the U.S. government in its decades-long "hot" fusion program, which sought to emulate the thermonuclear conditions in the cores of stars. Hot fusion had promised a distant era of safe, clean, infinite energy from the hydrogen isotope deuterium, which is abundant in water. These programs have resulted in useful plasma physics research, but no net energy release—ever. Thermonuclear bombs were far above "breakeven," but controlled thermonuclear fusion reactors at Princeton and at MIT were not.

Fleischmann and Pons were saying that they had achieved breakeven *already*, and, unlike hot fusion, there were no deadly emissions. The claim of a chemically-assisted nuclear fusion reaction with net energy release threatened to divert Congressional funding from the hot fusion program. With private zeal, and later public scorn, scientists supported by the hot fusion program—particularly at MIT—sought errors in the Fleischmann-Pons work.

When the exact radiation signatures and end-products of hot fusion reactions in a vacuum were not found in the Fleischmann-Pons results or in quickly-done tests at other laboratories, scientists at the MIT Plasma Fusion Center (PFC) yelled "possible fraud," "scam," and "scientific schlock." On May 1, 1989, the MIT PFC planted story in the press unleashed a torrent of anti-scientific bigotry. It did not occur to most scientists that a new class of nuclear reactions might have been discovered. As Nobel Laureate Julian Schwinger would say in a lecture at MIT in November 1991, "The circumstances of cold fusion are not those of hot fusion" (see pg. 81). He was ignored.

The furor over cold fusion in the spring of 1989 prompted President George Bush through Energy Secretary Admiral James Watkins to convene a "Cold Fusion Panel" of the U.S. Department of Energy's Energy Research Advisory Board (ERAB). Nobel Laureate Glenn Seaborg had told President Bush in the Oval Office on April 14, 1989 that the Utah discovery was "not fusion," thus poisoning the well and precluding an honest investigation. One of the twenty-three ERAB panelists had thought at the time: "Just by looking at Fleischmann and Pons on television you could tell they were incompetent boobs." (Prof. William Happer of Princeton, quoted by G. Taubes in *Bad Science*.) So much for the theory of the "unbiased" ERAB panel, which included Professor Mark Wrighton from MIT and the much less involved and (in 1999) apparently "neutral" Prof. Mildred Dresselhaus of MIT.

This panel, convened by the Department of Energy, was assigned to assess reports from various laboratories and to make recommendations to the U.S. government. Three major laboratories submitted negative reports. These were MIT, Caltech, and Harwell (England). The ERAB report was negative, and quickly so. A preliminary negative conclusion came in July 1989 and the final report November 1,

Editorial continued on page 4

New Energy Times Archive

Editorial continued from page 3

1989, with the following consequences: 1) No special funding by the U.S. government for further research; 2) Flat denial by the U.S. Patent Office of any application mentioning cold fusion; 3) Suppression of research on the phenomenon in government laboratories; 4) Citation of cold fusion as "pathological science" or "fraud" in numerous books and articles critical of cold fusion in general, and of Fleischmann and Pons in particular.

The 1989 reports of MIT, Caltech, and Harwell have each been analyzed by other scientists and these analyses have been published (see references, page 34). Each of the widely cited 1989 "null" experiments has been found to be deeply flawed in experimental protocols, data evaluation, and presentation. Each, in fact, contained some evidence of excess heat as claimed by Fleischmann and Pons. There is evidence that the MIT data was deliberately altered to erase an indication of excess heat. The altered data was published officially by MIT, and it was included in reports to a government agency under the official seal of MIT. The experiment was paid for out of federal government funds. This report had a dramatic impact on the perception of many scientists and journalists.

It is ironic that each of these negative results were themselves the product of the kind of low quality work of which Fleischmann and Pons were accused. The difference was that the reports said what the hot fusion community wanted to hear. This was the legacy of the 1989 ERAB report, but that legacy must now be reversed—and it will be, however long that takes.

Almost two years after they were concocted, Prof. Ronald R. Parker of MIT's Plasma Fusion Laboratory publicly stated that the MIT PFC cold fusion calorimetry data were "worthless" (June 7, 1991). In the same period (August 30, 1991) after I had challenged this data, Parker stated that "MIT scientists stand by their conclusions." Which is it?

The full story is given in detail in a "MIT and Cold Fusion: A Special Report" in this issue. I was there and I saw what went on—behavior far beneath what one would have expected from MIT. In 1991 I resigned my job in protest, and later founded this magazine. My 1991 book, *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor* (John Wiley & Sons) did not tell all that could have been told then. It took years to put it in proper perspective. Now the story has much more significance because Fleischmann and Pons have been vindicated—if not by the media and by the establishment, certainly by mountains of high quality published results.

We shall see what the MIT authorities of 1999 will do about the misrepresentations some of its staff made in 1989 and in the years thereafter. MIT continues to receive large Federal funding for its tokamak hot fusion project. In fact, as our Special Report reveals, MIT President Charles M. Vest is on a Federal panel that continues to recommend funding for tokamak fusion. The president of an institution as influential as MIT should weigh issues of intellectual integrity and conflict of interest very carefully. But past experience with Charles Vest and cold fusion, documented here, does not inspire confidence.

The literature on the Fleischmann and Pons effect is now voluminous—as most readers of *Infinite Energy* or *Fusion Technology* (an American Nuclear Society publication) know very well. It strongly suggests that what Fleischmann and Pons discovered was but the tip of an iceberg of a much more widespread phenomenon—"Nu-

Editorial continued on page 14

Institute for New Energy

is an international organization to promote new and renewable energy sources. Its monthly newsletter is

New Energy News,

reporting worldwide on all facets of new and enhanced energy. Memberships in INE are $35 per year for individual, $60 for corporations & libraries, and includes 12 issues of *NEN*

Journal of New Energy,

a Quarterly Scientific Journal
Hal Fox, Editor
Subscriptions are $150 for 4 issues.

Fusion Information Center
3084 East 3300 South
Salt Lake City, UT 84109

Also available, *Cold Fusion Impact in the Enhanced Energy Age*, a book about the near future of the world energy situation.

Phone for information: 801-466-8680

ENECO

ENECO is in the business of commercializing the exciting new field of low energy induced nuclear reactions in solids.

ENECO

University of Utah Research Park, 391-B Chipeta Way, Salt Lake City, Utah 84108 USA

Ph (801) 583-2000 Fx (801) 583-6245 jaeger@ENECO-USA.com

New Energy Times Archive

LETTERS TO THE EDITOR

To the Editor:

Thanks for another fascinating issue of *Infinite Energy*, which I am reading, and have got as far as pg. 6 with the letter from Jim Davis (Newbury, New Hampshire) and his mention of "eco-fascism."

We are already being subjected to this by politicians and environmentalists on the grounds that the supply of crude oil will run out shortly. In 1970 it was predicted that it would do this within fifteen years, but at present, known recoverable reserves are greater than they ever have been.

Anybody who believes that the oil will run out should be made to read Thomas Gold's book, *The Deep Hot Biosphere* (Springer, New York, 1999).

In this book, Gold describes how he drilled a borehole in Sweden and obtained oil from a depth of 8 kilometres (5 miles) beneath granite, a site where there cannot possibly be any geological organic remains to have given rise to the oil. According to Gold, the Earth is virtually saturated with hydrocarbons dating from when it first coalesced from the disk of primordial matter rotating about the Sun.

The presence of hydrocarbons in space is well-documented by radio-astronomy. Yet geologists cling to the dogma that all oil derives from organic matter from the geological past.

Living at great depth in the Earth's crust are bacteria which feed on the primordial organic matter, progressively reducing it from light to heavy hydrocarbons. This is why oil gives the appearance of being of organic origin.

Along with the oil, Gold recovered a substance which "stank like a dead rat on the garage floor." This can only have been the rotting remains of such bacteria.

The bacteria are still at work, and some oil field reservoirs are actually refilling, in the middle east and off the coast of Texas.

If Gold is right, the supply of crude oil and gas is effectively unlimited, and we can continue to use it freely until cold fusion is sufficiently developed. The oil can then be put to far better use as a chemical feedstock.

The eco-fascists, of course, are infuriated by Gold's theory and his proof of it with the Swedish borehole, as this will upset their plans to subject us to a society in which only the elite are allowed to drive cars.

So I would be grateful if you could give Gold's book some publicity.

George Sassoon
Wiltshire, England

✍ ✍ ✍

To the Editor:

Hello. My name is Alan Steenblock and I attend high school in Columbia, Missouri. I have been studying cold fusion as a hobby and have done experimentation with light water cells. For my school newspaper I am working on an editorial on cold fusion. I was wondering if someone could e-mail me back about the current status of cold fusion so I can put some quotes in my editorial. That way no one thinks I'm insane; well, they already think that I'm insane but. . . Perhaps someone could explain why the public doesn't know about cold fusion and new energy research, and "how come we don't have it yet." And any other information would be very useful. Thank you.

Alan Steenblock
Columbia, MO

✍ ✍ ✍

To the Editor:

I beg to differ with you about the heat balance of human metabolism. You were sure my estimate of 300 watts of radiated heat from an adult human being, normally clothed in the Occident, in a 20°C environment, is wrong (*IE* No. 23, p. 6). The 1985 ASHRAE Fundamentals Handbook, published by the American Society of Heating, Refrigeration, and Air Conditioning Engineers, gives specific values of body heat released, found in Table 18, Rates of Heat Gain from Occupants of Conditioned Spaces, on page 26.21. The lowest value stated is for seated at rest, as in a movie theater, which is 400 Btu per hour. This translates to 117 Watts. Walking at 3 mph is 1040 Btu per hour, which is 304 Watts. Heavy athletic activity is 2,000 Btu per hour, which is 585 Watts.

Since we only spend about eight hours per day asleep, the 117 Watts minimum in the ASHRAE table is the only significant daily activity which is less than this value. The other two-thirds of daily activities are at or above 117 Watts, some substantially above this. The table gives many more conditions; only three were cited to give the reader a perspective. A rough guess of an average daily heat dissipation of 150 watts is well-justified. I seriously doubt if the average American adult in a temperate climate takes in 3000 Calories in a day, which I have heard quoted for the winter diet of the Innuit, or Eskimo.

Once again, who wants to investigate?

Kenneth M. Rauen
W. Warwick, RI

✍ ✍ ✍

To the Editor:

I enclose a donation for your work on cold fusion. It is my habit to tithe one-tenth of my income every month to good causes and I can think of no better cause than cold fusion R&D. I hope very much that you succeed in your quest for a working prototype commercial cold fusion unit.

Anonymous

✍ ✍ ✍

✱✱ SPECIAL NOTICE ✱✱

IF YOUR SUBSCRIPTION BEGAN WITH ISSUE #19 AND YOU HAVE NOT YET RENEWED, THIS (ISSUE #24) IS YOUR LAST ISSUE OF *INFINITE ENERGY!*

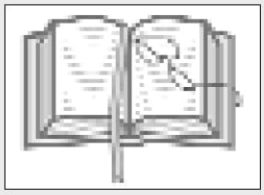
DON'T DELAY—RENEW YOUR SUBSCRIPTION *NOW!*

ENJOY ANOTHER YEAR (SIX MORE ISSUES) AT THE SAME LOW PRICE.

STILL ONLY $29.95
NORTH AMERICA
$49.95 FOREIGN

SEE PAGE 128 FOR renewal information

✱✱✱



Why not give *Infinite Energy* as a Gift Subscription to a friend or to your local library?

New Energy Times Archive

To the Editor:

How fascinating! I read with great interest the report by Dr. Gwinn of the University of California. (*IE* No. 18, pg. 23). He expounds on the reaction $Li + H^+ \rightarrow 2\,He$ So, no neutrons and no gammas, just a lot of heat and alphas that can easily be blocked.

I bring to your attention the fact that the Patterson Power Cell uses a 1.0 molar lithium sulfate electrolyte. (*IE* No. 1, pg. 21). We do recognize that the Patterson Cell produces excess energy.

The experiments run by Miles, Bush and Johnson for the Office of Naval Research used lithium deuteroxide for the electrolyte. That was a 0.1 molar solution. (*IE* No. 15/16, p. 39). These authors stress the correlation of excess energy and of production of excess helium, and conversely, no energy—no helium. Thus, their work sometimes worked, sometimes not.

Now I can't find earlier references to their first cell, but in the interview of Dr. Fleischmann by Chris Tinsley, we are told that in the original "cold fusion" experiments by Fleischmann and Pons, the electrolyte was lithium deuteroxide (*IE* No. 11, p. 18).

Permit me to suggest that other than supplying certain highly-activated catalytic centers, the palladium does not really participate in the excess energy production that has been reported by laboratory workers around the world.

I wish to suggest that it's really a nuclear reaction of the lithium alone. It is tempting to try to explain the fumble of MIT and other workers as having merely changed the obviously (!) unimportant electrolyte, and use (say) sodium or potassium salts. If a full description has not been published, someone would just have to go and ask them. With a change of electrolyte it's NOT really the SAME experiment.

Richard George
Downers Grove, IL

✍ ✍ ✍

To the Editor (from the online guest book):

I remember a few years ago that two physicists (Pons and Fleischmann) at the University of Utah discovered cold fission and later researchers at Texas A&M University said that they had confirmed that their experiment of cold fission was factual, despite the lashing they took in the press and in the scientific community. It is good to see someone going against the fossil fuel lobby in society. Keep up the good work.

Joe Van De Walle
San Antonio, TX

Editor's Note: You mean "fusion" not "fission" but ironically, some type of "cold fission" actually appears to be occurring too.

✍ ✍ ✍

Errata:

We normally print Errata, if any, in the next published issue. Since the errors noted below were just brought to our attention, we apologize for the delay in getting the corrections to our readers:

In Issue No. 21, page 54, the last line of "A Review of Perpetual Motion" by Mike Carrell was cut off and reads as follows: "Altogether, Collins' book is a coherent, useful contribution to the history of anomalous energy research and the human tragedy that often touches the pioneers."

In Issue No. 21, page 49, the empirical value of the Schwinger Ratio mentioned by Bob Bass is not "about 22" as indicated; it is "about 29."

In Issue No. 17, page 94, in reference to mathematical proofs which have not been published, Kenneth Rauen erroneously stated, "The processes of the first proof were selected only for their fully documented state variables of temperature, pressure, volume, enthalpy, and entropy at every point in the cycle." He has corrected that to note that "all points in the cycle in that mathematical proof have definitive state variables. The thermodynamic state variables which I transcribed from published literature were not complete. Some states lacked complete state variables, but in every case, two or more state variables were given, which is all that is required to define a state of matter, hence the name 'state variable.' With two state variables, only one state of matter will coincide with these conditions; it therefore is adequate to define a state of matter with only two state variables. From the data cited in my proof, all thermodynamic states are defined."

❑ ❑ ❑

***Infinite Energy* Solicits Your Manuscripts Dealing with:**

- Experimental results in cold fusion and new energy
- Theoretical Ideas
- Book Reviews
- Opinions

Submit to:
Dr. Eugene Mallove, Editor-in-Chief
or
Barbara DelloRusso, Managing Editor
at:
Infinite Energy Magazine
P.O. Box 2816
Concord, NH 03302-2816

Preferred format: Send double-spaced hard copy and disk. Software programs used to create equations should *only* be used for extensive equations separated from text. Please indicate what software/version (Microsoft, Windows, Word Perfect preferred), and platform (MAC or PC). Provide original hard copy of artwork. If you have questions regarding format or if you want to send paper by e-mail, please contact:

Barbara DelloRusso, Managing Editor
bdellorusso@infinite-energy.com

JOURNAL OF NEW ENERGY
A Journal of the Institute of New Energy Vol. 1, No. 1, 1996

Published by the
Fusion Information Center
3084 East 3300 South
Salt Lake City, UT 84109

A Quarterly Journal
Hal Fox, Editor

Subscription:
$150 for 4 issues
Single Issues: $45

THE MAGAZINE FOR IDEA PEOPLE
www.inventorsdigest.com
Inventors' DIGEST
Call today and take advantage of our no-risk subscription deal! We'll send you your first issue, and if Inventors' Digest is not what you need, just cancel your subscription. One yr. subscription: $22/U.S. $27/Can. $40/Overseas (U.S funds)
"In one issue of *Inventors' Digest*, I gained more knowledge about inventing pitfalls, marketing, patent searches, etc. than from all the other [business] magazines put together!" - *a new subscriber to ID*
1-800-838-8808
Inventors' Digest, 310 Franklin St., #24, Boston, MA 02110

Infinite Energy Press is interested in your book manuscript - please send draft manuscript or topic and short description, and estimation of completion time to the above address.

New Energy Times Archive

Cold Fusion: The First Ten Years

Looking Back And To The Future

Scott Chubb

Code 7252, Naval Research Laboratory, Washington, D.C. 20375-5351 USA

Tel: 202-767-5270, Fax: 202-767-3303; E-mail: chubb@ccf.nrl.navy.mil, chubb@neptune.nrl.navy.mil

"Science is magic that works." These words, by Kurt Vonnegut,[1] have power. They remind us of the mystery of science and that nothing in science is impossible.

Science should be like magic. But too often, it isn't. For too many scientists, science is little more than a form of livelihood. If we learn anything from the Cold Fusion (CF) controversy, I would hope it would be that science should be fun! It should be magical and alive! It should not be merely a business!

A second lesson of CF is the importance of language. From the beginning, incorrect language has plagued CF. The results have been catastrophic. The problems began when the BYU public relations department coined the expression "Cold Fusion,"[2] and Pons and Fleischmann (PF) decided to use the same name. This almost killed the field because many scientists assumed from the name "Cold Fusion" that PF had discovered a colder version of conventional fusion, while, at an early stage, it was obvious that this simply was not true. We now know that PF discovered a new form of room temperature, nuclear reaction, in which no high energy particles are released.[3,4]

The problem with the CF name touches on a deeper, subtler language problem, involving the clash between the paradigms[5] of nuclear and solid state physics. Specifically, nuclear physics relies heavily on paradigms involving localized, particle-like descriptions of physical processes. The paradigms of solid state physics are more "field-oriented," delocalized, and wave-like. An example of the resulting language problem is the failure by nuclear physicists to understand how a solid can trap the energy from a nuclear reaction. This behavior appears to be almost miraculous, in the context of the particle-like paradigms of nuclear physics. However, it is quite understandable in the context of the non-local, wave-like paradigms of solid state physics. The reason is that the field-oriented, wave-like picture that applies in an ordered solid is based on the idea that a periodic, static electromagnetic field can coherently transfer momentum, non-locally, without transferring energy.[6]

I'm optimistic. Compelling evidence[3,4] exists that PF Cold Fusion is quite real. Eventually, science will accept this fact and vindicate PF, but when? It could take a very long time. We can and must help to speed up the process. First, we must recognize the serious breakdown in communication that exists: although Cold Fusion is real, nobody is listening. Second, we must realize there is no simple cure for this problem: no single, smoking gun experiment or "commercial demonstration" will convince either the majority of scientists or society that Cold Fusion is real. Third, we must not give up. We can fix the problem. But we must be creative. We must become good teachers and listeners. We must change the focus of the debate. Possibly, we can do this in conventional ways. But we can also be innovative. For example, the Internet, schools, and even churches and synagogues, can provide avenues of communication that are worthwhile exploring.

We can and must re-awaken science to the reality of Cold Fusion. Science can be magic when it works. Let's make it work!

The Author

SCOTT CHUBB was born on January 30, 1953 in Manhattan, New York. He received his B.A. degree in physics from Princeton University in 1975 and his M.A. and Ph.D. degrees, also in physics, from the State University of New York (SUNY) at Stony Brook, in 1978 and 1982 respectively. His dissertation in the area of theoretical solid state physics and surface science was carried out primarily at Brookhaven National Laboratory (in Upton, New York). From 1982 to 1989, he primarily worked on problems associated with the electronic structure and magnetic properties of surfaces, interfaces and alloys, first as a research associate (from 1982-1985) at Northwestern University, in Evanston, Illinois, and then, while in residence at the Naval Research Laboratory (NRL), as a National ResearchCouncil Fellow (1985-1988), and as an employee of Sachs/Freeman Associates, Inc., of Landover, Maryland (between 1988 and 1989). Beginning in 1988, he also began work in areas related to applications of microwaves in space technology at NRL. He became an employee of NRL in 1989. Besides being a permanent NRL employee, concurrently, he has held consultant positions with Research Systems, Inc. (in Arlington, Virginia) since 1990, and Oakton International Corporation (in Oakton, Virginia) since 1995, in areas related to Cold Nuclear Fusion. His research interests include microwave sensing of the ocean, applications of relativity in the maintenance of precision time in space, and the study of hydrogen in metals and its implications in Cold Nuclear Fusion. He has been an author of more than fifty refereed publications in scientific journals, in areas related to solid state physics, general and special relativity, electromagnetic scattering, non-linear wave dynamics, statistical physics and nuclear physics. He is an author of one patent (awarded in 1996) for a device that eliminates navigational errors in the Global Positioning System (GPS) from the effects of the Sun on GPS microwave transmissions, and of two pending, process patents associated with Cold Nuclear Fusion. Scott Chubb is married to Anne Pond. They have three children, Scott Jr. (age 9), Lauren and Kathleen (both age 5).

Photo by E. Mallove

References

1. E.F. Mallove, *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor* (John Wiley & Sons, New York, 1991), iii.

2. S.E. Jones, private communication.

3. B. F. Bush, J. J. Lagowski, M. H. Miles, G. S. Ostrom, *J. Electroanal. Chem.*, 304, 271 (1991). M. H. Miles and B. F. Bush, *Fusion Tech.*, 25, 478 (1994).

4. Arata and Zhang, *Proc. Japan Acad.*, 73B, 1 (1997). Y. Arata and Y-C Zhang, *Proc. Japan Acad.*, 72B, 179 (1996).}

5. T. S. Kuhn, *The Structure of Scientific Revolutions*, Vol II, 10, (Univ. of Chicago Press, Chicago, 1962).

6. S. R. and T. A. Chubb, *Fusion Technology*, 24, 403, (1993).

New Energy Times Archive

Michael C.H. McKubre

Energy Research Center, PS385
SRI International
333 Ravenswood Avenue
Menlo Park, CA 94025 USA
e-mail: michael.mckubre@sri.com

A Personal View of Quests, Ghosts and Goals

The first decade of "cold fusion" coincides closely with my 40's—a decade where men traditionally grow from youth to maturity, becoming fatter and richer, complacent and affluent. The pursuit of this elusive beast we call "cold fusion" has, mercifully, averted both fates. The challenge announced on March 23, 1989, by the scientist whom I respected above all others, Martin Fleischmann, was one that I could not ignore. It allowed, indeed required, me to address my painstakingly assembled skills and expertise to a task that could make a profound difference to science and to humanity. What I did not appreciate was how much this quest would require me to learn: to unlearn and learn anew in my fields of expertise; to learn in new fields of which I was largely ignorant. While painful at times, my exposure to nuclear physics, experimental and theoretical, by itself was sufficient to avert complacency. Even better was the chance to meet, interact and work with some of the noblest and ablest people I have ever encountered or imagined existing.

Not all of the noble souls who rose with good intentions to the challenge have thrived in the academic Siberia to which we have been consigned. I will cite three whose contributions are unparalleled, but who have been particularly harshly judged and treated: my good friend and co-initiator of the quest, Stan Pons, Mel Miles, and Nigel Packham. Not all have survived, and I am burdened and blessed with three ghosts. Three individuals whose vision, technical talent, and charm were unequaled, but who did not survive to see the quest resolved. Here I will write about two whose contributions simply must not be forgotten: Andy Riley and Kevin Wolf. Andy contributed immensely to our understanding of the resistance/loading functionality for D/Pd, our calorimetric successes, as well as a plethora of other tasks and indispensable experimental knowledge. He was tireless, cheerful and deeply capable. I do not expect to see his equal again. Kevin Wolf was simply the best experimental physicist I have ever met. The result that he obtained, and tried to reproduce until his death, is the clearest and most unambiguous evidence of nuclear process and nuclear transmutation in the electrolytic D/Pd environment.

We must not forget, but we should not focus on the past. We have a diverse and international community of extraordinarily able individuals, sharing a common, if imperfect, understanding. It is the duty of every one of us to establish and work towards at least one goal that is achievable and meaningful. My goal some of you will find insultingly simple, some ironically amusing, and others naive and hopelessly ambitious. While my purpose is simple, to explain a set of observations for which no conventional explanation is apparent, my goal is "to prove unequivocally the existence of a new nuclear process in deuterated metal systems." Yes, we have amassed a great deal of evidence of heat and new nucleus production in deuterated (and even non-deuterated) metal systems, both in the electrolytic and gas phases. It has often been said that this evidence in particular instances or generally is "overwhelming." Well, neither the scientific or public communities have been overwhelmed, and it is we who are at risk of being submerged. For a proof we need evidence that is easily understood, unarguable and absolutely solid. To obtain this proof we at SRI are pursuing closely the experiments of Professors Arata and Zhang in the electrolytic system, and Les Case in the gas phase.

It has been a long road, tortuous, often dark and occasionally painful. I would not have chosen any other course and could not have found better companions. The end is not in my sight but the path is clearly marked. Let us walk it with vision and with pride.

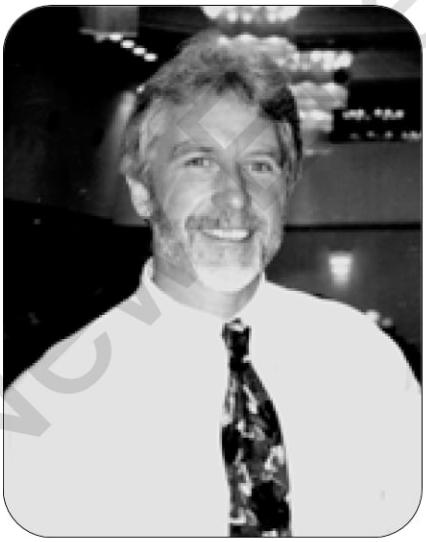


Photo courtesy M. McKubre

The Author

MICHAEL C.H. MCKUBRE received his B.Sc. in chemistry and physics (1971), M.Sc. (with honors) in geophysics and physical chemistry (1972), and a Ph.D. in chemistry (1976), Victoria University of Wellington (New Zealand). **Specialized Professional Competence:** Electrochemistry; surface and membrane chemistry; AC properties of heterogeneous electrolyte systems; steady-state and transient properties of porous and flow-through electrodes; electrochemical sensors; battery systems; metal-hydrogen systems; corrosion science; expertise in the fields of geophysics and electronics. **Representative Research Assignments at SRI (since 1979):** Studies of the sulfur electrode in support of Na/S battery development; development of thermodynamic models; identification of kinetic limitations; development of corrosion resistant coatings; development of sodium/metal chloride high energy density batteries; studies of the thermodynamics and kinetics of the absorption of hydrogen isotopes by Pd; development of microelectrochemical probes for the study of localized corrosion; development of Harmonic Impedance Spectroscopy to characterize the non-linear response of charge transfer process in Faradaic and corrosion reactions. **Other Professional Experience:** Lecturer in electronics and surface chemistry at Victoria University of Wellington (New Zealand); studies of the origins of electrical polarization within the earth and of membrane polarization in general Postdoctoral Research Fellow at Southampton University (England); studies of porous electrodes to characterize the dispersion of potentials using low-frequency AC methods and computer modeling techniques Program Manager of Electrochemistry at SRI 1982 - 1990. **Professional Associations and Honors:** Member of the Royal Chemical Society; member of the Electrochemical Society; past editor of *Electrochemistry* for the National Association of Corrosion Engineers. **Publications:** Two theses, five reviews, more than ninety papers in scientific journals, books, conference proceedings, and published technical reports.

New Energy Times Archive

George H. Miley

University of Illinois
103 S. Goodwin Ave.
Urbana, Il 61801 USA
Tel: 217-3333772;
E-mail: g-miley@uiuc.edu

First, on behalf of myself and the researchers at the Low Energy Nuclear Reaction (LENR) Laboratory at the University of Illinois, I wish to thank the staff of *Infinite Energy* for organizing this issue commemorating the tenth anniversary of the cold fusion announcement by Pons and Fleischmann. This startling announcement caught the attention and imagination of numerous people, and provided a challenge that was taken up by researchers worldwide. However, the ensuing controversy drove many persons away. Last year I had the experience of giving the first seminar on the subject at the University of Utah's chemistry department since Pons and Fleischmann departed some years ago. Some young faculty had never even met them. Yet, I found emotions among the staff were still running high even after all this time, mainly due to outside criticism that still pours in. That is only a sampling of the situation. By boldly continuing to cover cold fusion and related fields despite this continuing turmoil on all fronts, *Infinite Energy* provides a major driving force for alerting people worldwide to the importance of developing new energy sources to sustain and expand mankind's standard of living. If new sources are not found, not only will living standards suffer, but war and chaos may well develop as nations strive to compete for remaining scarce natural energy resources. On a longer time scale, new energy sources are also key to future space exploration, both for local sites (*e.g.* a lunar mining base) and for major missions beyond the lunar missions (*e.g.* a manned mission to Mars).

Unfortunately, the present temporary surplus of oil, coal and timber has tended to lull our government and others into complacency, as illustrated by the skimpy research budgets for studies of future energy sources at DOE, NASA and NSF, as well as private companies. The U.S. does not plan ahead in things like this (there is no official "long range energy plan"), rather we react to market pressures. In the past, when a crisis arose, when our "back was put to the wall," we have always been able to turn things. The shift from a manufacturing-based industry to an information-based society is a recent example—as our steel mills shut down, the needed innovation for change largely came from individuals and small companies rather than from a government plan. But now, with the amazingly fast pace for world affairs, we may not have time to do the necessary energy research to turn things around if it isn't seriously underway in advance. On the bright side, *Infinite Energy* has a strong role in starting a movement among individuals and small companies that could provide the "ground swell" of enthusiasm for energy research needed to save the situation. Time will tell—but time flies! It seems like only yesterday that I received an announcement of the first issue of *Infinite Energy*. Energy research must exponentiate to keep pace if future events go as I see them. (To be open about this, I must admit to having a long term bias—over fifteen years ago in the section on professional goals in my "Who's Who" biography I wrote that my goal was to help "insure that future generations have a plentiful supply of economical, readily available energy. . .")

LENR Laboratory's Staff at the University of Illinois, Nuclear Engineering Department. From left, Prof. George H. Miley; Giovanna Selvaggi, Research Assistant, graduated from the University of Rome "La Sapienza," Italy; Mike J. Williams, Senior Electronics Engineer; Andrew N. Tate, Research Assistant, graduated from the University of Missouri - Rolla; Maria Okuniewski, Research Assistant, graduated from the University of Tampa - Florida. *Photo courtesy G. Miley*

Where does cold fusion fit into this picture? Clearly, if it proves to be real as many of us believe, cold fusion would fill many of the future energy needs I am talking about. I might say "most" needs, but how such power sources scale in size and exactly what "fuels" (natural resources) are consumed must be settled to fully understand this. Unfortunately, research in the area has been much slower than originally anticipated by Pons and Fleischman and other pioneers. I need not elaborate the reasons to *Infinite Energy* readers. No one anticipated the complexity of the problem. The early statement that any high school student could build and operate a cold fusion experiment must rank with the statement that nuclear fission would produce electricity "too cheap to meter" as classic mistakes. The polarization and viciousness that developed around the field, both by "outsiders" and "insiders" has been much more severe than could ever have been predicted. Instead of people being drawn into cold fusion research by the exciting new science involved, they were driven away by the attacks on the field and on individuals. Fortunately, I believe things are now turning around. The fascination of the emerging basic science and technology, plus the importance of the field to society, has kept many going. Now, as new results emerge, new young scientists and engineers are beginning to look at the field afresh, without fear of criticism or ridicule. This is, in my view, a time of consolidation within the field. Soon we will see a new exponentiation of activity.

Several factors are coalescing to drive the field forward. First, work on the traditional "Pons-Fleischmann" effect has shown the parameter range needed to successfully study excess heat phenomena. This requires a complex combination of loading, deuterium flux, surface conditions, etc. Unfortunately, early experimenters didn't realize this, so many experiments were done in regimes where nothing should have happened, and didn't! That confusion

Miley continues on page 10

The Author

GEORGE MILEY is Professor of Nuclear and Electrical/Computer Engineering at the University of Illinois, U-C Campus. He is well known in the cold fusion area for his research on LENRs, and as editor of *Fusion Technology*, an ANS publication. He is also editor of two other professional journals, *Laser and Particle Beams* and *Journal of Plasma Physics*, published by Cambridge University Press. His research accomplishments in hot fusion, lasers, and energy conversion have been recognized by Fellow appointments in three different professional societies: APS, ANS and IEEE. Other honors include the Edward Teller Medal, ANS Fusion Research Achievement Award, Guggenheim Fellowship, and the Halliburton Engineering Education Leadership Award. He is a consultant at several national laboratories and serves on various State and Federal scientific committees. He is dedicated to student teaching and has supervised a number of MS and Ph.D. theses in a wide variety of topics.

New Energy Times Archive

Keith Johnson

Quantum Energy Technologies Corporation
238 Main Street, Suite 400
Cambridge, MA 02142 USA
e-mail: kjohnson@qetc.com

The tenth anniversary of the discovery of cold fusion is of great significance to me on two levels. First, I became personally involved as an MIT professor in attempting to provide a theoretical basis for the phenomenon shortly after witnessing the televised press conferences of Pons and Fleischmann in the spring of 1989. I had been working for some time on a theory of high-Tc and other anomalous superconductors, based on the quantum chemistry and nonlinear vibronic (dynamic Jahn-Teller symmetry-breaking) properties of materials. I had previously published an invited paper in a special issue of *Synthetic Metals* 5, 151 (1983) in which the unusual superconducting properties (*e.g.* inverse isotope effect) of palladium loaded with hydrogen or deuterium (heretofore unexplained by BCS theory) were included in my theory. What struck me immediately about the Pons-Fleischmann claims was the fact significant excess heat and very low-level bursts of neutron emission identifying their cold fusion phenomenon occurred only when the loading was approaching one interstitial deuterium atom per palladium atom. This was also the deuterium concentration at which palladium, which in its pure form is almost a ferromagnet, becomes a superconductor at the relatively high T_c of 10° K. I was certain that the dynamic Jahn-Teller vibrations of interstitial deuterium atoms in palladium

Photo, MIT News Office

(forming the "gamma phase" of PdD) induced by the calculated electronic degeneracy at the Fermi energy and triggering the superconductivity of PdD, also played a role in the cold fusion phenomenon. We published our first paper on cold fusion shortly thereafter (K.H. Johnson and D.P. Clougherty, *Mod. Phys. Lett.* B3, 795 (1989)), and later a paper at the Maui Cold Fusion Conference, in which the "excess heat" and very low levels of D-D fusion were tied respectively to the gamma phase transition and dynamic Jahn-Teller D-D vibrations between tetrahedral interstitial sites of the PdD gamma phase. Since my theory of superconducting PdD also explained the lower Tc of 9° K for PdH (the inverse isotope effect), I predicted in 1989 that excess heat should also be produced in Pons-Fleischmann experiments using only ordinary "light water," albeit with smaller excess heat liberation relative to "heavy water" and with no H-H fusion. The production of excess heat in light-water cells was later corroborated in several laboratories.

The second area of my life concerned with cold fusion was the writing of a screenplay for a feature film titled "Breaking Symmetry." Having witnessed both the science and politics of cold fusion at MIT, I completed this "science-fiction" screenplay in 1993 about what happens to a young female astrophysics professor and expert on cosmic dark matter after she discovers a conspiracy to sabotage cold fusion research at a renowned technical institute. The screenplay attracted the attention of the Creative Arts Agency in Los Angeles, which after wide circulation of the script in Hollywood quickly elicited interest in producing "Breaking Symmetry" as a feature film at a major Hollywood studio. However, the failure to get an agreeable studio development deal and the later coincidental appearance of unique plot and character elements of my script in several major Hollywood features motivated me to produce and direct the film myself in the Cambridge-Boston area, as I had originally envisioned it. We shot the film over six weeks in 1996 and I completed post production of "Breaking Symmetry" this past year. Showings of the film at festivals and in theater distribution is expected this year—the tenth anniversary of cold fusion.

PROF. KEITH JOHNSON now works at Quantum Energy Technologies, Inc. a Cambridge, Massachusetts-based company that is working on the augmentation of fossil fuel energy with advanced technologies. QET is also exploring technologies that use water alone as fuel.

Miley continued from page 9

caused the famous non-reproducibility problem and lead a number of experimenters to give up. Another extremely positive aspect of the "traditional" experiments has been the continued search for helium "ash," with several laboratories now reporting quite convincing positive results. The presence of this "ash" is the "tell-tale" sign of a nuclear reaction, and provides a benchmark for theoretical studies.

A second unexpected factor that has emerged to excite people about the field is the discovery of transmutations in special cold fusion configurations. This effect is now termed Low Energy Nuclear Reactions (LENRs), hence the name for our Laboratory at the University of Illinois. Like traditional cold fusion, LENRs have been thrown into an arena of intense controversy that tends to distract from research, rather than seeking to understand the phenomena. But, as more independent laboratories continue to report LENR experiments, this area too is slowly attracting attention by serious scientists, both in and outside of "traditional" cold fusion research. Since in LENRs, the electrode material itself is involved, LENR type cells could be used for transmutation products, or, as some are investigating, even for nuclear waste treatment. Equally importantly, heat cells appear possible, and the study of LENR "ashes," like studies of helium "ash" in traditional cold fusion, provides a most important way to understand and optimize heat producing reactions. LENR and cold fusion reactions do not represent separate fields, but branches of the same field of nuclear reaction in solids or condensed matter. Consequently, the growing interest in each reinforces the basis for my prediction that the whole field will shortly "take off." It also provides theorists with yet another challenge, namely explaining the connection between the two by development of an unified theory.

Scientific communication is key to rapid growth of a scientific area. *Infinite Energy* and several other journals such as the *Journal of New Energy* play a most important role in this respect. Also *Fusion Technology*, which I edit, continues to publish peer reviewed research papers in the area. The ICCF international meeting series has also had a strong impact. I served as scientific program chair for ICCF-7, held last year in Vancouver, so I am best aware of the details of that meeting. In my opinion, the quality of the presentations and proceedings has continued to improve, and the meetings are reaching out to new people interested in the area. A

Miley continues on page 14

New Energy Times Archive

Francesco Celani

Instituto Nazionale Fisica Nucleare
Via Enrico Fermi,
Frascat, 00044, Italy

I started cold fusion experiments just one week after the announcement of Martin Fleischmann and Stanley Pons. The main reason, personally, was to disprove the "funny" results of nuclear measurements (neutrons).

At that time, we had just finished "qualifying" the Underground Gran Sasso Laboratory (Italy) where the neutron emission was, and still is, extremely low. We developed, for this kind of measurement, special equipment that gives reliable performance.

Indeed, accurate measurements (inside this low-background laboratory) of the Fleischmann-type cell gave us some results about weak neutron emission. We realized they were not the result of errors.

Even in the "primitive" apparatus used for electrolysis, we realized that the weak neutron emission appeared only when two conditions were fulfilled:

a) large loading of Pd (we used, at that time, ultra-thin and long tubes); the qualitative check of loading was performed by observation of overvoltage of the Pd-Pt cell at the times of floating conditions and

b) there was some induced non-equilibrium conditions about concentrations of deuterium inside Pd.

Later on, we continued this exploratory study using even high temperature superconductors—deuterium gas loaded at high temperature. As a gift, we obtained the proof that superconducting critical temperature of YBCO compounds increased when loaded with deuterium in the proper amount. The paper about this unexpected result was published by the prestigious journal, *Physics Letters A* (other specific experiments followed, with four papers in total published from our group in *Physics Letters A* about this topic).

After three years of "underground life," we moved back to "normal laboratory life" at Frascati, in order to produce the excess heat experiments developed by A. Takahashi in Japan. We made some improvements on his procedure (new calorimetry) and I got the chance to join an international (Japan, U.S., Italy) effort to reproduce and understand this kind of strange phenomenology. During this work, I was in contact with Tanaka K.K. Japan Company, which provided all the groups with the Pd plates used for heat measurements.

The contact person at the international level was Dr. Missa Nakamura, whom I met in 1992 for professional reasons. In 1996, we happily decided to marry. . . Another unexpected result and nice gift from cold fusion!

After replication of the Takahashi experiments, I decided to improve his procedure even on the loading aspects. I developed the so-called "high-power, high-frequency pulse loading" that gives us, as main results and apart from excess heat, the following unexpected results:

1) evidence for a new metastable phase of Pd-D at high loading (published in *Physics Letters A*) and

2) indications of a possible superconducting state when loading is made extremely large by the use of very high frequency excitation (about 81 Mhz).

Very recently, we explored the effect of controlled impurity additions on thin Pd wires in an acidic high-resistivity electrolytic solution. We were able to get, in a reproducible way, values of H/Pd extremely large and stable over time. As a gift, perhaps we understood the true complicated electrochemical effects that trigger to the over-loading of D/Pd and subsequent excess heat phenomenon discovered by Fleischmann and Pons. It seems that we are able to get full control of the overloading procedure at this time with hydrogen. (Paper to be submitted to *Physics Letters A*.)

As a final impression of ten years of cold fusion, I can say: I looked for something and found only partially what I wanted and. . . as gifts I got other things more interesting and useful. I would like to continue to study cold fusion and related topics as long as possible because I like good, unexpected situations in my life!

Photo by E. Mallove

The Author

FRANCESCO CELANI was born in Rome, Italy on February 8, 1951. Experimental Physics degree received from the University of Roma "La Sapienza," 1975. Staff member/researcher of INFN-LNF, 1976 - present. Involved in an International Collaboration for experiments (1976-1982) in High Energy Physics (20-300 GeV at CERN (European Center for Nuclear Research), Geneva (Switzerland). From 1989 - present, Dr. Celani has been studying Cold Fusion phenomena; From 1989-1992 inside the Gran Sasso Underground Laboratory, searching for nuclear ashes (mainly neutrons); later on searching mainly for excess heat, by flow-calorimetry, at LNF. Key point of their research was and still is trying to find proper procedures in order to reduce the irreproducibility behavior of these kind of experiments.

New Energy Times Archive

Dennis Cravens

P.O. Box 1317
Cloudcroft, NM 88317 USA
E-mail: xyzxxzyx@hotmail.com

Lessons Learned

I have learned a lot over these last ten years. Mostly they have been things that don't work. Working with nickel/water and spiked water systems has taught me that those systems serve to give only short-lived, low-density heat. They also yield lower percentage gains as you go to higher currents. These are serious engineering challenges. Much solid research needs to be done before commercialization can be considered. Palladium/deuterium systems have higher energy densities and scale better at higher currents and temperatures. I have learned that commercialization must follow extensive research.

I have been constantly amazed at people rushing off to see a quick effect, but never seeking help from those working on the systems. It seems that patience and primary source research is still the missing ingredient for success. Palladium needs a long, slow, patient loading for it to yield a rise in the baseline after calibration heat pulses before it is ready for high currents. Afterward, slow cycling between low and high currents then can give better response, and this conditions the metal. The palladium systems have taught me patience, long suffering, and to trust only first hand information from those doing the work in the labs, not their handlers. You can learn a lot by asking.

Higher temperatures during running seem to give better responses, yet many researchers try to run at the convenient room temperature ranges. I have learned to look for the system's own internal preferences instead of what is convenient in the lab.

Wire cathodes load higher than plates and thick rods. I have learned that thin is good and fat is bad.

Small impurities are not always bad. Slightly impure Pd has better crystalline stability. A little Ca and Al can increase loading. Boron leached from glass can seal loading levels. I have learned that a little can be good, but a lot can be bad, and that timing is important.

If you watch in real time, you can see voltage fluctuations before heat production. You will not see it on the averaged data from a computer file. Also, good Pd has a distinctive bubble production between high/low switching. I have learned that you can see a lot by looking. I have looked for lots of neutrons but never find any. I have rushed to false conclusions.

I have hoped for fast solutions. What I have learned is that nature has its own rules and we must play by them and not our own assumptions. She reveals her secrets slowly if we but listen.

The Author

DENNIS CRAVENS (Ph.D. Florida State University 1977) is a retired college professor. His cold fusion work from 1989 to 1995 focused on conditions required for producing excess heat (see premier issue of *Infinite Energy*). His work indicates that long loading at low current density should precede any high current application to the palladium. From 1995 to 1998, Cravens has worked as a consultant in the area of nickel/water systems and has pointed out that the heat gain percentages drop off at higher current densities in such systems. His current work again focuses on conditions, such as pulsing the current, application of RF, and additions of Ca and Al, that initiate the production of excess heat and permit higher loadings.

Photo by E. Mallove

John Dash

Physics Department
Portland State University
Portland, OR 97207 USA

Electrolysis of Heavy Water with an Acidic Electrolyte

Photo courtesy J. Dash

In April 1989, my department head asked me to try to reproduce the results reported a few weeks earlier by Fleischmann and Pons. We had previously studied the electrolysis of water with an acidic electrolyte, so we used the same composition, except that heavy water was substituted for light water in the electrolyte. Using a small (about 1 sq. cm), cold rolled palladium foil cathode about 25μm thick, we observed macroscopic plastic deformation of the cathode soon after the start of electrolysis. I had never seen such behavior in my thirty years of research on electrolysis, so I was immediately intrigued and have remained so for the past, exciting ten years. Students, who are quite open-minded, have also been intrigued. Five M.S. theses and one Ph.D. dissertation have been completed on this research, and twenty high school students have performed experiments during summer apprenticeships. We believe that an acidic electrolyte inhibits the deposition of impurities and enhances the deposition of hydrogen isotopes on the cathode, in comparison with the basic electrolyte used by many investigators. The use of thin foil cathodes reduces the time required to saturate the cathode with hydrogen and deuterium, so it is possible to demonstrate an excess heat effect in hours rather than days. We also commonly observe localized concentrations of unexpected elements, in particular silver, on the surface of the palladium after electrolysis. Using mass spectrometry, we found inversions of palladium isotope concentrations on the surfaces of palladium cathodes after electrolysis, compared with these concentrations on unelectrolyzed palladium from the same batch. In addition, microscopy and x-ray fluorescence studies of palladium cathodes have shown that changes in surface topography and composition continue to occur long after electrolysis ended. In recent years we have been studying the use of titanium cathodes in an acidic electrolyte. We find excess heat and localized concentrations of unexpected elements (usually vanadium, chromium, and iron) on the titanium cathodes after electrolysis. By mass spectrometry, we also find changes in isotopic abundance, compared with the natural abundance. For both palladium and titanium, it appears that excess heat is associated with hot spots, which are marked by localized concentrations of unexpected elements. We believe that the amount of excess heat is directly related to the number of hot spots. The challenge we have is to determine what the characteristics of the metal (thickness, grain size, purity, etc.) must be in order to maximize the number of hot spots. Our research would not have been possible without the support of the Portland State University Physics Department, the Academy of Applied Science, Mr. and Mrs. Jerome Drexler, Mr. and Mrs. Charles Entenmann, the

Dash continues on page 14

New Energy Times Archive

Peter Glück

Institute of Isotopic and Molecular Technology, Cluj-Napoca, Romania
E-mail: peterg@oc1.itim-cj.ro and peterngr@mail.soroscj.ro

My First Ten Cold Fusion Years

Cold fusion will change the lives of billions of people by assuring them independence and prosperity regarding energy. This belongs to my professional credo. However it also had changed the lives of a selected group of people, even before it was accepted and applied.

I belong to this group. Cold fusion is a unique intellectual and sentimental adventure. My life was intensified, my feelings of hope, disappointment, love, and hate became more vivid. I will live probably five years less than it was planned. But I don't regret it at all because I got the opportunity to live a real peak experience. No other scientific field could do this for me and for my friends. I was there from the very start.

After some thirty years of work, in 1989, based on my experience in research and development, I stated that progress is inherent to technology. Hot fusion seemed to contradict me! When I heard about the historical press conference of Fleischmann and Pons, I said "Ecce!" (See!--obviously I said it in Romanian), this is the solution of the fusion problem and I became totally dedicated to CF. And finally, too, a devilish cycle of great expectations and giant disappointments started; however, after each wave, or ebb and tide, more and more certainties have accumulated and the light(s) at the end of the tunnel are visible.

Photo by E. Mallove

And the field grew and grew and grew beyond any expectations and prediction. Many diverse systems and some ten times more theories appeared in many parts of the world. It is a Herculean task to find some unity in this diversity. The field is cursed with irreproducibility; however, I think this is actually a blessing because it demonstrates that the multiplicity of phenomena takes place in elusive, strongly localized, active sites.

Seven years ago I published a paper about cold fusion being the extreme form of catalysis and catalysis being the common factor of all the systems—of those viable and up-scalable at least. My part of truth has to fight with other parts of truth, claiming they are the complete, whole, total explanation of everything. I think today one of the great tasks is to do "integration management" and "knowledge management" in the field. Cold fusion has lost its identity or has expanded it beyond many limits, has no more definition; there is no holistic and holographic vision of it. As a marvel of synchronicity, the cold fusion revolution was associated with a political change and with the Internet renaissance giving us "communication beyond geography, collaboration beyond history, information beyond informatics." All we lack now is a bit more understanding and open-mindedness.

The story of cold fusion demonstrates this. Anyway, I am focusing on the future and I am looking with anticipated joy to my next ten cold fusion years.

The Author

DR. PETER GLÜCK is a chemical engineer with almost forty years of practice in research and a pioneer of the consulting activity in his country. His credo is based on the "useful trinity—science, technology, and management." He believes management is a basic issue in solving the problems of cold fusion. He is a proponent of a kind of technological optimism—that is technological progress is unstoppable and always wins.

BART SIMON

Department of Sociology
Queen's University, Kingston, Ontario
K7L-3N6 Canada
Tel: 613-533-6000 x77152
Fax: 613-533-2871

A Sociologist's View

For a phenomenon that is not supposed to be real, scientific research on cold fusion has a surprising longevity. The last ten years have brought us a steady stream of experimental data, new problems and ideas, new skills and techniques, and a plethora of theoretical innovations (some more plausible than others perhaps). Despite the curious and arrogant disapproval of the few skeptics who still bother to comment, there is still cause to investigate the phenomenon. There may yet be no pot of gold at the end of the rainbow, but genuine knowledge is being produced—no one should disapprove of that.

This said, it is clear that the last ten years have not brought us two things. The first is that we are still lacking a stable, reproducible anomalous phenomenon, a reliable "black box" device that everyone has access to from Gene Mallove to Michael McKubre to Steven Jones. Claims of such black boxes have surfaced from time to time, and I have no reason to doubt their veracity, but I do doubt the extent of their distribution. Private experiments cannot generate public knowledge, and only public knowledge can make cold fusion real.

The second thing we are missing is a coherent and directed research program. Without the organizational support of mainstream institutions, journals, and funding agencies, CF research has splintered into a dozen different directions. At conferences some similarities in the various methods emerge, but genuine collaboration is rare. This is unfortunate. Bill Collis' idea for an international cold fusion society is a good one. Take it from a sociologist, without some form of collective organization we may be forced to wait for that one black box that makes cold fusion real and it may be that one black box that we do not even recognize.

Photo courtesy B. Simon

The Author

BART SIMON is an assistant professor in the Department of Sociology at Queen's University in Kingston, Ontario, Canada. Bart teaches on the social aspects of science and technology and his recently completed dissertation is on the sociology of the cold fusion controversy. He is currently working on the manuscript for a book on the importance of the cold fusion case for the sociology of scientific knowledge.

New Energy Times Archive

Akira Kawasaki

Los Angeles, California USA
aki@ix.netcom.com

Congratulations to *Infinite Energy* on its anniversary issue. How time flies! Going into the fifth year of publication eh?!

In this field, there are many people of diverse background that became attracted and involved since the announcement made by P&F in 1989. I am one of them and a senior citizen now, although I do not feel like one.

I am a wayward "research physical chemist-engineer" by personal preference. No, I do not have a lettered degree in the profession. My thirty something years of work was in retailing/real estate by the natural course of family exigencies. But a lifelong devotion to science exists.

I was still in the tail end of running a retail business when the March 23, 1989 P&F announcement was heard. It was exciting, thrilling and made eminent sense to me. Inasmuch as I was still involved in working seven days, no vacation work cycle, going on some twenty years, there was hardly any time to pursue the revelation other than to scan the newsstands and mentally cheer on the CF effort. It was hard on my family.

By 1993-1994, I had retired from active retail business and was still adjusting to the absence of the seven-day work week. Management of small properties remained. So what to do with time? Go back to science and read, particularly the CF field. This was when the original *Cold Fusion* magazine was started with Eugene Mallove as chief editor. This was when *21st Century Science and Technology* magazine had coverage of cold fusion. I subscribed to both. Later, when Gene started *Infinite Energy*, I was happy to help him get the first issue of *Infinite Energy* out in time for the Fifth International Conference on Cold Fusion. This was after first helping him videotape his first cold fusion seminar at MIT in January 1995. I felt that archiving the seminar and these conferences were important. It has been an interesting experience.

I take all the research and experimentation undertaken since 1989 to be the prelude to the harnessing of cold fusion in all forms of excess energy production.

"Cold Fusion" was not expected to take this long and longer. However, to understand and control this "paradigm shift" discovery, it is/was found that basic research pointing to the causes were not fully available in past references but had to be found and developed as cold fusion research itself went along. And we have progressed closer to defining and understanding "cold fusion."

And all this occurred during the past ten years without major domestic public funding. Nothing to brag about but there were a few people that believed in cold fusion enough to continue on in private despite adversities. Dr. Eugene Mallove is among a handful. Final results are bearing out their convictions.

And the cold fusion field, once derided after initial ill-prepared and ill-understood disappointing results of the phenomena, stands ready to be finally recognized and developed as a legitimate field of science and energy resource for the next millennium and beyond. Ten years is a short time (but too long on a human scale).

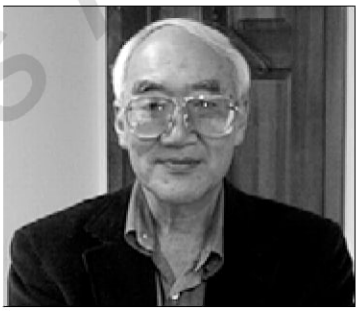


Photo by J. Rothwell

Dash continued from page 12

New York Community Trust, the Naval Research Laboratory, and the U.S. Army Research Office. Finally, I am very grateful to Professors Fleischmann and Pons for revealing their discovery to the world. The future of cold fusion is very bright. We have only seen the tip of the iceberg.

The Author

JOHN DASH, Professor Emeritus, Physics Department, Portland State University. Education: Ph.D., Metallurgy, Pennsylvania State University, 1966. Employment: Physics Department, Portland State University, 1966-1998, specializing in physical metallurgy and electron microscopy. Since June 1998, he has been involved in research.

Miley continued from page 9

significant number of attendees at ICCF-7 had not been at prior meetings. I am confident that the next meeting in Italy will continue to grow in all of these respects. Also, regional meetings, *e.g.* in Italy, Russia, and one sponsored by *Infinite Energy*, have been growing and provide another important opportunity for scientific interchanges. Along these lines I would note that special sessions on LENRs have been introduced into annual meetings of the American Nuclear Society (ANS). The first two of these, sponsored by the Radiation and Isotopes Division, have been very successful. Consequently, the division program committee has scheduled a third special session for the November 1999 ANS meeting in Los Angeles. I hope to see many *Infinite Energy* readers there (and at other cold fusion/LENR meetings).

Editorial continued from page 4

chemistry," if you wish. Selected papers are cited in this issue as a starting point for those who need to study some of the best hard evidence. These are not fantasies. This is solid work, the kind of pioneering, exhaustive experimentation that could have been done at places such as MIT, Caltech, and Harwell, but wasn't.

The production of excess heat in the range of hundreds of megajoules per mole of metal has been confirmed, as well as the production of helium, tritium, and other elements. Power densities of kilowatts per cubic centimeter of electrode have been achieved by some researchers. The field of Low Energy Nuclear Reactions has been established, if not yet widely recognized. Low energy neutron or weak gamma radiation are seen in some experiments, but most produce excess heat with no radiation or radioactive byproducts. Rapid remediation of radioactive materials has been demonstrated. What a fantastic opportunity for universities such as MIT to become involved!

The replication and commercial application of the Fleischmann and Pons effect has been inhibited by a lack of understanding of the exact nature of the reactions, which are not those known to plasma physicists. There is a severe and widespread materials and theory problem related to cathode materials that produce the effect. Criteria are available to test cathode materials for potential activity, but knowledge of how to produce such material at will is not available.

Sad to say, solving the materials problem may be beyond the financial resources of the scattered researchers who have worked to validate the Fleischmann and Pons effect. Unfortunately, the negative reports by key hot fusion laboratories to ERAB prevented diversion of government funding from the failed hot fusion program to the more promising field of cold fusion. The patent-crushing ERAB report also became a severe deterrent to private

Editorial continues on page 39

New Energy Times Archive

Harold Aspden

Energy Science Ltd.
P.O. Box 35, Southampton
S016 7RB England
h.aspden@physics.org

Ten Years of Cold Fusion:
Or Was it Ten Years of "Cold War?"

I recall the excitement forty years ago in 1958 when those working under official secrecy admitted defeat in their efforts to tame the discharges in what was to be a hot fusion reactor. By their public disclosure they sought inspiration from the scientific community. The electrodynamic pinch could not bring those heavy ions, be they protons or deuterons, close enough together in that hot plasma discharge for long enough to trigger their fusion. The miniature sun they were trying to create would not light up!

Yes, the destructive power of the hot fusion reaction had been demonstrated, the miniature "supernova" of the H bomb, but the task of replicating the process as a controlled heat generation means that could provide us with electrical power was proving impossible.

Thirty years on, with many billions of dollars having been spent, the dream of hot fusion power was clearly still in the throes of a slow but inevitable death. Then along came Martin Fleischmann and Stanley Pons, presenting experimental evidence which implied that those deuterons could get together close enough to

Photo courtesy H. Aspden

generate an excess of heat if only they were invited to meet up inside a host metal cathode in an electrolytic cell at water temperature. This gave reason to believe that the generation of useful power from nuclear fusion could be possible given moderate and sustained research funding aimed at "cold fusion" rather than "hot fusion."

The hot fusion community was beside itself, outraged at the audacity of such a prospect. There was a conflict of interest, tempered by disbelief, and so we have witnessed a chapter in science that is quite shameful, besides being severely detrimental to our quest to find new non-polluting sources of energy.

What are the lessons learned from this saga? What happens now? The hot fusion solution is dead, from natural causes. The cold fusion solution seems to have been killed by an act of murder, but there was a time when it had the breath of life and we can but hope that its' seed may propagate and recover what has been lost.

A drastic shake-up is needed to get energy science back on course. There were some voices in the scientific community those forty years ago telling us that the heat generated by the sun may not involve "hot fusion." They were ignored but maybe the core of the sun is not as hot as we imagine. Maybe there are processes inside the sun that create an environment analogous to that of the host metal cathodes in the experiments of Fleischmann and Pons.

My message and indeed my efforts in this area have concentrated on deciphering the evidence from a broad spectrum of physical, cosmological, and technological phenomena that will help in that drastic shake-up. I can but refer to what I have recorded on my web site www.energyscience.co.uk\coldfusion.htm and a final comment of relevance. I had no problem whatsoever in securing the grant of British Patent No. 892,333 for a hot fusion reactor when I filed on that, back in February 1958. However, when, in April 1989, I filed on cold fusion, I ran up against the brick wall built by the U.S. Patent Office, using bricks supplied by hot fusion specialists. It took me over nine years to tunnel underneath that barrier and to secure U.S. Patent No. 5,734,122 based on that 1989 filing.

The Author

HAROLD ASPDEN was born in England on December 12, 1927. At the age of 17, he entered Manchester University and graduated with First Class honors in Electrical Engineering in 1948. After graduate shop floor training in the power industry from 1948-1950, he conducted research at Cambridge University for a Pd.D. thesis on the anomalous power loss in electrical sheet steel and is author of several papers on the subject. From 1953-1959, he worked in the Patent Department of the English Electric Company, and was inventor on several patents. In 1960, he became IBM's U.K. Patent Manager and from 1963-1983 their European Patents Director and European Patent Attorney. This included voluntary service for a time as Rapporteau for the Paris-based International Chamber of Commerce on patent-related topics and also a stint as President of the Patents, Designs and Trademarks Federation in the U.K. Aspden is author of several books and papers on topics connecting electrodynamics and gravitation, including the aether and its energy aspects. In 1983, he retired early from IBM and with IBM sponsorship took up a visiting research appointment in the Electrical Engineering Department at the University of Southampton in England. He became interested in the prospect of "free energy" in 1986, including thermoelectrics and later cold fusion, especially as the latter presented challenging patent opportunities. In 1997, he established a website (www.energyscience.co.uk/coldfusion.htm). Dr. Aspden claims to have discovered how the proton is created from the quantum activity of the sea of energy that pervades the aether, a finding which, in 1975, gave its theoretical mass at 1836.15232 times that of the electron and which, because it was so close to the value ultimately measured some ten years later in 1985, was remarked on as being "curious" by those who measured this quantity. No biographical note concerning Dr. Aspden's career can escape mention of his insight into the role which aether energy plays in creating matter, as evidenced by the way in which it regulates the values of the fundamental constants of physics. Aspden notes, "There is no way in which I could have earned my living and developed this aether energy picture had I remained in the academic world. The critics would not have tolerated it and, as it is, they just ignore what I say and in doing so ignore the energy prospect simplified." He and his wife, Wendy, married in 1954 and will soon celebrate their 45th anniversary.

New Energy Times Archive

Hal Fox

Trenergy, Inc.
3084 East 3300 South
Salt Lake City, UT 84109 USA
Tel: (801) 466-8680 Fax: (801) 466-8668

Photo by E. Mallove

Ten Years of Low-Energy Nuclear Reactions

[Note about Author: On the day that Professors Pons and Fleischmann announced the discovery of cold nuclear fusion (March 23, 1989) Hal Fox decided to become involved. With participating friends, the Fusion Information Center was formed and incorporated by mid-April, 1989 with offices at the University of Utah's Research Park. Since that time, over 3,000 papers have been obtained, read, and reviews written and published in the first cold fusion newsletter, *Fusion Facts*, and later in *New Energy News*. Important original articles and proceedings of conferences have also been published in the *Journal of New Energy*. In July 1999, Hal Fox will have spent ten years at editing these various publications reporting on cold nuclear fusion and other new energy discoveries.]

When Professors Stanley Pons and Martin Fleischmann made the announcement of the discovery of cold fusion (March 23, 1989), their professional report had already been written and sent to the *Journal of Electroanalytical Chemistry*. The original paper suggested that the observed results may be the fusion of heavy hydrogen (deuterium). This announcement of low-energy nuclear reactions has resulted in a decade of investigations by over 200 laboratories in thirty countries. In addition, over thirty theories have been advanced to explain the many (over 600) papers reporting on reproducing or expanding on this initial discovery. One of the first to replicate Pons and Fleischmann's discovery was the famous electrochemist, Professor John O'M. Bockris. Bockris was the first to replicate Pons-Fleischmann's experiment and to measure the production of tritium.

Dr. Randell Mills soon announced the discovery of significant excess heat from the electrolysis of nickel cathodes in a light-water solution. Later, Bush and Eagleton, and Swartz provided firm substantiation of excess heat produced in light water which could best be explained by nuclear reactions.

The first known report to identify the highly unusual and scientifically unexpected anomalous production of nuclear reactions involving heavy elements was by Rolison *et al.*[1] in 1990. Rolison, in her presentation of anomalous changes in the isotopes of palladium (reporting evidence of nuclear reactions found at the Naval Research Laboratory), was immediately corrected by an MIT scientist who assured her that such nuclear reactions were impossible. Had this pioneering work been accepted as a valuable scientific discovery (and properly funded), the current discoveries and culmination of the understanding of nuclear reactions initiated by electrochemistry could have been shortened by five years.

With the immediate dismissal of any possibility of nuclear reactions of heavy elements in electrochemical experiments, the potential rapid advances in low-energy nuclear reactions were almost abandoned. However, by 1996, the evidence for a variety of low-energy nuclear reactions was so compelling that Professor John O'M. Bockris organized the first Low-Energy Nuclear Reaction Conference. The conference was held at Texas A&M, where Bockris was a distinguished professor. The proceedings of the conference were published as Volume 1, Number 1 of the *Journal of New Energy*. A second conference was held the following year in College Station, Texas (but not on the Texas A&M campus) with the proceedings being published in the *Journal of New Energy*. During this second conference, Kenneth Shoulders presented a paper to explain the role of high-density charge clusters in nuclear reactions in deuterated metals.

The discovery of high-density charge clusters has been a part of public information since 1991. However, it was not until Shoulders presented his paper at the second Low-Energy Nuclear Reaction Conference that the role of charge clusters in nuclear reactions had been made public. At the same conference, Mizuno presented a paper on measurements of extensive changes in isotopic ratios of elements found in palladium after electrolysis. Later, Mizuno wrote an excellent personal history of his scientific investigations of low-energy nuclear reactions.[2] At this second Low-Energy Nuclear Reaction Conference, George Miley presented an array of experimental evidence for nuclear reactions occurring in thin-films.

In Chapter 7 of Mizuno's book, the author presents information provided by E. Conte, an Italian scientist, who claims to have a theory that explains many of the observed effects of low-energy nuclear reactions. The basis for Conte's explanation is his work on Biquaternion Quantum Dynamics and the application of his theoretical work to experimental findings in low-energy nuclear reactions. Conte's work is worthy of serious consideration.

What Has Been Learned in Ten Years?

Although cold fusion is widely

Fox continues on page 17

The Author

HAL FOX is an unretired 75-year old scientist. Born and raised in rural Utah, Hal has seen the world grow from horse-drawn implements to placing a man on the moon. With a strong interest in science from the time he could read, Hal has been variously, an enlisted person in the Army Signal Corps (World War II, Philippines), a fixed-station telephone repairman, and a meteorologist in the Air Force Weather Service (regular officer during Korean and Viet Nam fracases). Hal says he resigned his commission because the weather didn't agree with him. Hal became a missile-system engineer (Hughes), a member of the Advanced Engineering Department (Sperry), and the director of the first research laboratory at the University of Utah Research Park. His next self-appointed task was to become a computer programmer, President of CAI (Computer-Aided Instruction) where his group developed educational programs beginning with the Apple 2-E (using COBOL). "We would go to lunch while a program was compiling." Hal turned 65 just before the announcement of the discovery of cold fusion. He and friends started the Fusion Information Center at the University of Utah Research Park. Hal and staff have collected over 3,000 papers on cold fusion, from over 200 laboratories in 30 countries, all reported on in *Fusion Facts* (the first publication for cold fusion and now part of the *Journal of New Energy*). Hal wrote one of the first books about cold fusion: *Cold Fusion Impact in the Enhanced Energy Age* (1992). Hal and staff have collected over 1,000 papers on "new-energy devices and systems" and publish *New Energy News* and the *Journal of New Energy*. His hobbies are reading, writing (133 listings in the *Fusion Facts* bibliography), and organic gardening.

New Energy Times Archive

Mahadeva Srinivasan

15, Rukmani Street,
Kalakshetra Colony
Besant Nagar, Chennai, 600090, India
chino37@hotmail.com

For those of us who have had the opportunity of witnessing the Cold Fusion Saga from a ring side seat, ten years does appear "long." Most of us were optimistic that we would be able to crack at least the physics or science behind it by now, even though scaling-up and commercialization might take more time, or so we thought. But the field has turned out to be even more baffling and mysterious than we imagined it to be, when we first plunged into it in the spring of 1989. Every one of us who got "sucked" into it has experienced both the excitement of obtaining positive new results revealing some new facet of the phenomenon as well as the frustration and disappointment that comes from being unable to reproduce those results at will! Clearly nature has still the upper hand in this field. Looking back I for one do not ever remember having doubted the reality of the phenomenon, for we had seen both neutrons and tritium in a variety of experiments. But the field has been continuously changing complexion like the proverbial chameleon—from Pd-D_2O to Ti-D_2 gas to nickel light water to plasma discharge to ceramic conductors and so on—the sands have been ever shifting! There has naturally been an inherent inertia to assimilate the

Photo by E. Mallove

new findings and to acknowledge that the phenomenon is more complex than "mere fusion of deuterons" in the heart of a host metal lattice—that indeed the unthinkable was happening, namely transmutation of the host metal nuclei to new elements! Slowly it dawned on us that it is verily alchemy come true. In fact, besides Kervran's biological transmutation, and George Oshawa's carbon arc induced transmutations, even alchemy recipes right out of ancient palm leaf manuscripts have been resurrected and thrown into the melting pot! The milieu that has emerged now goes by the broad title of "Low-Energy Nuclear Reactions."

But more than the physics behind the phenomenon, it is the human drama, the behind the scene conspiracies, the scheming by vested interests, the mental block of the scientific establishment, the fear of ridicule by fellow scientists, the secrecy due to patent considerations and the lure of the pot of gold at the end of the commercialization rainbow, all this has been an eye opener for me.

Yet with all this there is a vibrant core of dedicated experimenters and theoreticians still carrying on, in spite of the lack of funding. I am confident that some one will soon come up with a reproducible experiment which can be switched on at will to show to the skeptics that Low-Energy Nuclear Reactions do occur in specially prepared experimental configurations. Once that happens, funding will flow and young students will be able to do graduate work and the field will take off much as any other branch of scientific inquiry. Prototype devices for heating purposes will follow. I have no doubt that the new millennium will see the emergence of this new technology in the marketplace. But in my opinion the impact of the paradigm shift needed to understand the new phenomena will have even more far reaching consequences.

The Author

MAHADEVA SRINIVASAN received a B.Sc.(phy), University of Madras, 1955, B.Sc.(Tech), University of Madras, 1957, First Batch of BARC Training School 1957-58, International Institute of Nuclear Science and Engineering, Argonne National Laboratory, Illinois, USA 1961-62, M.Sc.(phy) University of Bombay 1967, and a D.Sc(phy) University of Bombay, 1985. His research career spanned four decades at Bhabha Atomic Research Center. He has specialized in the Physics of Fission and Fusion Reactor System, was responsible for the design, construction, operation and experimentation with a variety of experimental reactors, both fast reactors and thermal reactors, fueled with U-235, Pu-239 as well as U-233. He was head of the Neutron Physics Division and subsequently Associate Director of the Physics Group of BARC. Dr. Srinivasan was also involved in cold fusion experiments right from its inception in 1989, playing a leading role in the BARC studies in this area. Since retirement in 1997 (from BARC), he has been living in the southern Indian city of Chennai and serves in an advisory capacity to cold fusion researchers in India.

Fox continued from page 16

believed (mainly by those who have not read the literature) to be non-existent, the scientific truth is that low-energy nuclear reactions (including those found in cold-fusion experiments) are real. Here is a summary of some of the experimental discoveries:

1. The initial Pons-Fleischmann effect has been replicated and/or improved as reported in over 600 papers from over 200 laboratories in 30 countries.

2. The fusion of deuterium, when it occurs, is different than deuterium fusion in plasma physics. Specifically, neutrons are not emitted as expected. However, there is strong scientific evidence for the production of both tritium and helium.

3. Although more improbable than heavy-water nuclear reactions, light-water excess heat has been discovered and replicated.

4. Unusual nuclear reactions have been observed in plasma devices.

5. Nuclear reactions occur in gaseous experiments using palladium.

6. Nuclear reactions occur in special types of proton conductors.

7. Anomalous nuclear reactions have been found using high-voltage discharges onto target elements.

8. Dozens of reports have been made of anomalous isotopic shifts in palladium and in a variety of elements that are produced during low-energy nuclear reaction experiments. See especially Mizuno and Miley's work.

9. Arcing under water using highly-pure carbon electrodes has produced iron and the isotopic ratios are not normal.

10. Charge clusters can be produced in light-water solutions by using "valve metals" and produce nuclear reactions. This was first reported by Stan Gleeson and Rod Neal (the so-called Cincinnati Group) and further studied by the author and others.

11. Using low-energy means (high-density charge clusters), table-top particle accelerators are now possible.

Fox continues on page 22

New Energy Times Archive

Robert W. Bass

P.O. Box 1238
Pahrump, NV 89104 USA
E-mail: donquixote@radix.net

The epochal discovery of induction of nuclear reactions by low-energy means, as in table-top electrochemical processes, is destined to have a greater impact on human civilization in the twenty-first century than even electronics, computers, and information-processing technology has had on the final decade of the twentieth century.

We live in the century of Cybernetics, but we are on the threshold of the century of Ergonics.

In fact, when fully understood, developed and utilized industrially and for the benefit of individual consumers, the new scientific "continent" (rather than island), as Mizuno wisely calls it in his indispensable new book, *Nuclear Transmutation: The Reality of Cold Fusion*, will be regarded ultimately as the most significant scientific discovery in recorded history: one with literally Millennial import. Accordingly, the Swedish Royal Academy (keepers of the Nobel Prizes in Physics, Chemistry, Peace, etc.) does not have enough awards in their entire cupboard to adequately honor the paradigm-shattering discovery that Martin Fleischmann and Stanley Pons announced just one decade ago this month.

Although Fleischmann and Pons are destined to join the permanent pantheon of humanity's greatest heroes, the taxpayers have not yet realized how they have been betrayed by a selfish scientific oligarchy more concerned with retention of its present privileges than in the traditional scientific goal of the honorable objective search for truth. In the long run their ignoble refusal to consider the evidence will do to the scientific Establishment what the Inquisition's silencing of Galileo did to the late-medieval Church. The Cardinals who refused to look through Galileo's telescope for fear of jeopardizing vested interests are no different from those in today's high-energy physics and hot fusion research communities who have insisted upon classifying Cold Fusion as "pathological science" and have intimidated their students from researching it and

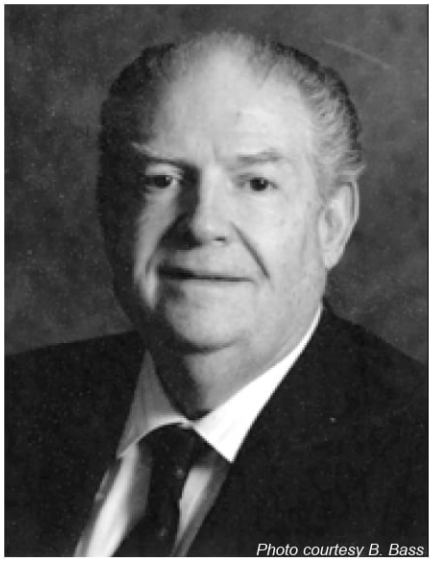


Photo courtesy B. Bass

have intimidated the pack-mentality media into failing to bring the truth to the attention of the public.

The greatest living futurist (and, in 1945, inventor of the geo-stationary communications satellite), Sir Arthur C. Clarke, in 1998 stated that the suppression of Cold Fusion is the greatest scandal of the twentieth century. No less than four Nobel Laureates in Physics have disagreed with the claim of the DoE's ERAB Report that Cold Fusion is scientifically impossible: both Julian Schwinger and Willis Lamb have published theories of how conventional QM can predict CF inside of periodic lattices; while Harvard's Norman Ramsey publicly refused to sign the ERAB Report until a sentence was added which admitted that the entire report could be mistaken, and Cambridge theorist Brian Josephson, while pinpointing weaknesses in Schwinger's work in privately faxed communications, has added that he is prepared to allow for the possibility that the phenomenon may be genuine.

The real problem is that the existing Peer Review System has become irredeemably corrupt, as many Establishment insiders have long admitted privately. (The late Richard Bellman started four new journals in applied mathematics and related subjects in science and engineering, none of which utilize anonymous referees, because, as he told colleagues, the existing system of anonymous referees had become hopelessly corrupt.) To verify that it is not merely Bellman who had reached this conclusion, consult the website (http://www.caltech.edu/~goodstein) of Caltech Vice Provost and physics professor David Goodstein (a founder of the field of Condensed Matter physics who recently received the Oersted Prize from the AAPT) and, in addition to his enlightening essay "Whatever Happened to Cold Fusion?" read his obituary for the present peer review system entitled "Conduct & Misconduct in Science." "The whole system of peer review is in peril. … [R]eferees are able, with relative impunity, to delay or deny funding or publication to their rivals. … [I]t is the referee who is guilty, but it is the editors and program [funding] officers who are responsible for propagating a corrupt system that makes misconduct almost inevitable. Scientists are not disinterested truth-seekers, they are more like players in an intense, winner-take-all competition for scientific prestige or perhaps merchants in a no-holds barred market-place of ideas."

Although Goodstein does admit, "I suppose that, if nuclear fusion really does take place whenever [cathode loading] is greater than 0.85 in palladium, the world of conventional science will eventually be forced to take notice," in an earlier version of this paper, published in *The American Scholar* under the title of "Pariah Science," his concluding sentence lamented that, regardless of the facts, "no one is listening."

This de facto censorship by the Establishment will be seen by later generations as having constituted a major crime against humanity.

In fact, in order to explore Mizuno's "new continent" adequately to unravel all of its complexities, and to discover

Bass continues on page 21

The Author

BOB BASS is an internationally-recognized inventor and former Professor of Physics and Astronomy (BYU, 1971-1981, retired). He has served on the faculties of Aerospace Engineering Sciences (University of Colorado), Electrical Engineering (UCLA), and Aero Engineering (Egyptian Air Academy). In addition to seminal issued Patents, he has over forty years experience in university teaching, research, and in industrial R&D in the areas of modeling, analysis, computer simulation, empirical parameter-identification (PID), and control algorithm development for aerospace vehicles and other complex dynamic systems, such as fusion plasmas. Postdoctoral, Princeton University 1955-56, 1957-59; Ph.D. (Mathematics), Johns Hopkins University, 1955; M.A. Oxon (Maths), Oxford University (Rhodes Scholar), 1952; B.A. (Physics) John Hopkins University, 1950.

New Energy Times Archive

Edmund Storms

2140 Paseo Ponderosa

Santa Fe, NM 87501 USA

Rejection of CANR has been premature. The cold fusion debate has struggled on for almost ten years with essentially no progress in converting skeptics to believers. Indeed, the attitude of conventional scientists has hardened. On the other hand, new demonstrations of the claims have appeared, reproducibility has been achieved using several methods, and a relationship between heat production and appearance of a nuclear product has been determined. All of these successes were demanded by skeptics before claims could be accepted. So why has this acceptance been denied?

Two major reasons appear to be driving the present attitude. First, the original work was very incomplete in its execution and unconventional in its revelation. Understandably, many influential scientists immediately concluded the claims were nonsense without waiting for more information. Second, the claims violently conflict with conventional models of nuclear interaction. People who have a commitment to these models, generally physicists, are no longer willing to reexamine the field in light of new, more complete information. Instead, all kinds of objections are proposed in a desperate effort to denigrate the data, no matter how carefully obtained. It has been my experience that these objections are generally based on a failure to understand the methods used in the experiment, a failure to understand that many of the suggested errors were acknowledged long ago and removed, and a failure to consider the insignificant magnitude of the suggested errors. The result is that asking critics to carefully examine cold fusion today is rather like asking the President to define sex.

Photo by E. Mallove

The major conceptual errors driving skeptical attitudes are:

1. The nuclear mechanisms and products produced in a cold, solid, periodic environment will be the same as found in a hot, chaotic gas, *i.e.* cold fusion must be identical to hot fusion. Numerous models have now been proposed to show how these two environments can give different results.

2. The deuterium nuclei can approach no closer than the separation measured in β-PdD, one of the presumed active materials. This distance is clearly too large to permit nuclear interaction. On the other hand, good reasons now exist to believe another hydride phase, in which the nuclei are much closer than previously thought, is the actual active material. In addition, the process clearly involves a more complex process than simple close approach.

3. Claims for excess energy production are rejected based on errors applicable to absolute calorimetry. Because relative calorimetry is actually used, many of these suggested errors do not apply. In addition, most of the suggested errors, including the absence of stirring during the original studies, have been shown to be insignificant.

4. The amount of anomalous heat has dropped as the skill of the experimenters has increased. This myth is based on ignoring all variables except time and ignoring all work except that done using the Pons-Fleischmann method. In fact, success continues to increase using eight other methods now known to produce the effect.

5. Helium can leak from ambient air into an apparatus through which gas is flowing and be mistaken for anomalous helium. This problem has been eliminated by using all-metal systems, by looking for other components arriving with the air, and by doing duplicate studies using apparatus of different designs. In addition, recent studies have detected anomalous helium at concentrations well above that in ambient air.

6. Modern, well accepted tools for detecting elements at low concentrations are believed to routinely give false values when applied to a CANR experiment. This problem is proposed to show itself no matter where in the world the tools are used or by whom, no matter how experienced the operator. This attitude is also applied to measurements of tritium and neutron emission.

7. All people working in this field are incompetent because some poorly done studies can be cited, *i.e.* guilt by association. A less charitable corollary is all people reporting positive results are incompetent.

8. All data not peer reviewed is junk and not worth considering. Never mind that most peer reviewed journals will no longer accept a cold fusion paper for peer review. In the past, only a fraction of available papers were accepted.

9. Science needs to be protected from the damage "bad" science may do to its reputation. Presumably, excess skepticism does no harm.

I suggest the field has been prematurely rejected and now deserves a second chance to prove its worth. To do otherwise is to deny mankind the potential for clean, unlimited energy and science the possibility to understand a new mechanism of nuclear interaction.

The Author

EDMUND STORMS obtained a Ph.D. in radiochemistry from Washington University (St. Louis) and recently retired from the Los Alamos National Laboratory after 34 years of service. His work there involved basic research in the field of high temperature chemistry as applied to materials used in nuclear power and propulsion reactors, including studies of the "cold fusion" effect. Over seventy reviewed publications and monographs resulted from this work as well as several books, all describing an assortment of material properties. He presently lives in Santa Fe where he is investigating the "cold fusion" effect in his own laboratory. These studies have resulted in fourteen publications including three complete scientific reviews of the field, one published in 1991, another in 1996 and the final one in 1998. In May, 1993, he was invited to testify before a congressional committee about the "cold fusion" effect.

New Energy Times Archive

Tadayoshi Ohmori

Catalysis Research Center,
Hokkaido University
Sapporo, 060 Japan

Expectation for the Future of Cold Nuclear Reactions

In my opinion, 1998 was very fruitful in that the reality of the cold fusion reaction was nearly proven by several experiment observations reported in the ICCF-7 conference: intense excess energy generation, new element production, plasma emission, x-ray emission, etc. Let us hope that this year will see the first efforts at making a commercial prototype nuclear energy reactor.

In 1992, several researchers observed excess energy evolution in Ni/ H2O electrolysis systems. Based on this, I suppose that the major nuclear reaction is not necessarily the conventional D-D or D-T reaction, but perhaps some other reaction involving protons or H atoms. In 1996, this concept was validated by discovery of anomalous reaction products inside and on the surface of Au, Ni and Pd electrodes after electrolysis in H_2O and D_2O electrolyte solutions. These products included, for example, Fe, Hg, Ni, Si, Mg, etc., with isotopic shifts quite different from the natural distribution. The appearance of a group of volcano-like structures on the Au electrode after electrolysis shows the active points where the nuclear reaction takes place. In addition, as we reported at ICCF-7, up to 200 watts per cm^2 of the excess energy was generated by a cathodically electrolyzed W electrode, in incandescent condition. This improves the prospects for energy generation at the industrial level.

We have proposed various nuclear reactions including fusion and fusion to explain the anomalous products. However, the key question—"what is the initial inducing nuclear reaction?"—remains open. At the present stage, we are assuming the reaction, $H^+ + e^- \rightarrow$ neutron induces the reaction, although we grant that this theory seems recklessly bold. This is based on the idea that the electrons ejected out of the potential well of the electric double layer by tunneling are captured by the nuclei of H^+ or H atoms present at the outside of the double layer. If this assumption is valid, the neutrons thus produced would be captured by the electrode material atoms, inducing further nuclear fusion or fusion reactions to form the Hg, Fe, Ni, and so on, found so far experimentally. In most cases, proton captured electrons form H_2 via H atom formation. Perhaps only a small fraction of the H+ or H atoms are thought to be involved in the neutron production. The neutrons thus produced would have small kinetic energies, which would allow them to partake in the nuclear reaction effectively, emitting little radiation to the surrounding environment.

The experimental apparatus we use in our incandescent W experiment is very simple. It requires only an electrolytic cell, working electrode material, Pt mesh as the counter electrode material, a thermometer and electric power supply. However, materials must be extremely pure, since the generating ability of electrode material is severely hampered by contamination.

In the future we expect to see the development of a theoretical interpretation of these results.

Photo courtesy T. Ohmori

The Author

TADAYOSHI OHMORI is a research associate at the Catalysis Research Center of Hokkaido University (B.S., Chemistry, Hokkaido University, Japan, 1962; Dr., electrochemistry, Hokkaido University, Japan, 1988). His current research interest is interfacial energy conversion and characterization of active sites of the electrode.

R. A. Oriani

University of Minnesota
Corrosion Research Center
Minneapolis, MN 55455 USA

Progress in enhancing the generation of thermal energy and in understanding the underlying mechanisms has been disappointingly slow since 1989. In my opinion, several phenomena have been established. These include the possibility of generating more thermal energy than the energy supplied and that the excess cannot be understood by chemical reactions, the possibility of generating helium and tritium, and the possibility of generating elements that were not present in the specimen prior to the experiment. The word, possibility, is carefully employed here because each of these phenomena cannot be reproduced at will. These experimental facts abundantly justify intensified investigation not only because of their intrinsic scientific interest but also because of their technological potential.

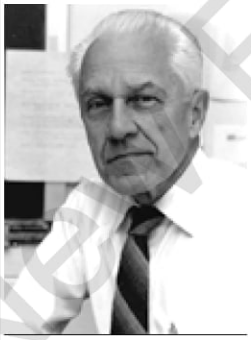


Photo courtesy R. Oriani

Credibility of the field has been hurt by a large number of poor, uncritically done experiments and by a large number of poorly considered theoretical approaches. Additional damage has been done by the uncritical presentations of poorly done work and unsupported claims by some magazines that aim to represent the field. Their polemical tone does not help either. One should keep in mind the dictum that controversial findings and ideas need very strong evidence to be believed.

Future progress demands extremely careful work by experimentalists especially in attempting to establish relationships between magnitude of excess thermal energy and magnitudes of helium, tritium, or other elements not initially in samples. Theorists should take special pains to specify how an experimentalist can disprove their theories, and the experimental irreproducibility must be explicitly addressed by any theory that attempts to explain these phenomena. People who claim that their device or process can produce excess energy reliably should, after patent application, if desirable, make their "recipes" or devices openly available to others.

The Author

RICHARD A. ORIANI, Professor Emeritus (1989-present), University of Minnesota, Director, Corrosion Research Center (1980-1987). Research interests: Themodynamics of alloys, diffusion, thermomigration, electromigration, nuclear magnetic resonance in alloys, nucleation, internal oxidation, Ostwald ripening, thermodynamics of elastically stressed bodies, solute-defect interactions, hydrogen embrittlement of steels, impact sticking, stress corrosion cracking, hydrogen permeation, corrosion, electrochemical passivation, cold fusion. Over 160 scientific papers and three books.

New Energy Times Archive

John O'M. Bockris

4973 Afton Oaks Drive
College Station, TX 77845 USA
Tel: 409-776-6139; Fax: 409-776-0298
E-mail: bockris@myriad.net

Not New. When Fleischmann and Pons came out with their famous announcement, the general idea was that this was taken as original work. However, papers of cold fission have been published since 1943.

What has been established? Formation of tritium, generation of helium at a value of about half that necessary to explain the heat. Irreproducible production of heat in bursts. Up to 1 kW per cc (thin films). Metal-to-metal transmutation. Irreproducible production of noble metals from cheap materials by impact method; production of new metals in palladium containing high concentrations of hydrogen or deuterium.

Where is the frontier? (1) In metal-metal transmutation, (2) in transmutation in biological organisms.

What is the main problem? Reproducibility.

Importance of damage. In palladium, internal damage begins at the extraordinarily low overpotential of 0.3V and is visible after twenty minutes. It occurs at the grain boundaries. The damage is concomitant with reactions which cause the production of new materials, or is it the production of new materials which cause the damage? I tend to favor the former theory, though admit that there is evidence for "hot spots" associated with damaged areas.

Impurities in solution. I never saw any positive results occur if the solution was pure. It is better to have an impure solution, *e.g.*, one to which thiourea has been added.

Do nuclear reactions occur in biological organisms? This, of course, is much less well-established than the work in metals. It must be regarded as very tentative. There is the early work of Kevran but more recently Komaki has put forward reports which seem convincing.

The theory of irreproducibility. Hardly any theories account for the reproducibility. In my opinion, the most important aspect of any theory of low-energy nuclear reactions must be to interpret this characteristic, irreproducibility. The damaged area must contain many points of what chemists would call active sites, *i.e.*, atoms with unsaturated valences. It is possible that at these points the nuclear reactions occur, be they neutron transfer reactions, D-D collisions, or whatever.

Political side. The politics of low energy nuclear reactions is negative because of pressure from the scientific establishment. They will do everything possible to negate the funding of research because of the implication that their scientific judgment is not to be trusted.

Short term future. Everything depends now upon the reproducible reaction and funding by NSF and the like. One of the biggest burdens that the subject has had is that it has been the province of chemists and electrochemists. However, George Miley is a powerful and influential nuclear engineer. If he publishes papers which have the same material in them as those of myself (historically), Mizuno, etc., it will be more influential in the U.S. than the witness of a physical electrochemist and a Japanese nuclear engineer.

Long term future. The discovery of low-energy nuclear reactions is on a par with the discovery of nuclear fission by Hahn and Meitner in 1939. It may be that we have the nuclear power source we need. But whether this will be in five or fifty years, a great deal will depend on the dying off of the old physicists, and their replacement by a group willing to face facts.

Photo courtesy of Texas A&M University

The Author

Dr. John O'M. Bockris was a Distinguished Professor of Chemistry at Texas A&M University (1982-1997). His research interests are in the areas of quantum electrochemistry, photoelectrochemistry, electrocatalysis, bioelectrochemistry, corrosion, the splitting of water, and low-temperature nuclear reactions. Bockris was born in Johannesburg, South Africa in 1923. He received a B.Sc. degree in Chemistry with First Class Honors from the University of London in 1943, the degree of Ph.D. in 1945, and the degree of D.Sc. in 1952. Upon receiving his Ph.D., Bockris was appointed to the faculty in Chemistry at Imperial College and by 1948 had a team of ten graduate students and some postdoctoral fellows. He is a founder of the International Society for Electrochemistry, started in 1949. Bockris organized a significant research group in electrochemistry at Imperial College, working particularly on the basic techniques of measurement of electrodes, and also in the field of high-temperature liquids. He emigrated to the United States in 1953 and in 1954 was appointed as a Professor of Chemistry at the University of Pennsylvania. During the 1960's, Bockris led the largest electrochemical group in the western hemisphere. His best known book, *Modern Electrochemistry*, was written with A.K. Reddy during that time and published in 1970. In 1972, Bockris was appointed to the Chair of Physical Chemistry at Flinders University in South Australia. There he became the Chairman of the Institute of Solar and Electrochemical Energy Conversion and wrote the book, "The Solar Hydrogen Alternative." In 1974, with others, he founded the International Society for Hydrogen Energy. In 1979, Professor Bockris was made a member of the Swedish Academy of Engineers. Also in 1979, he became the first recipient of the "Faraday Medal Award" by the Faraday division of the Chemical Society. He was given the "Chemical Lecture Award" of the Swedish Academy in the same year and was the recipient of the "Medaille d'Honneur" from the University of Louvain. In 1986, we was given an honorary doctorate from the National University in La Plata, Argentina. In 1982, Bockris was appointed a Distinguished Professor at Texas A&M University and in 1983 became the Director of the National Science Foundation's Hydrogen Research Center. In 1988, he received the "ACS Award for Contributions to the Chemistry of Contemporary Problems" and in 1989 the "German Augustin-Mouchot Prize for Contribution to the Solar-Hydrogen Economy." Among other more recent awards, he has been given the membership of the Serbian Academy of Science; the "Honoris Causa Award" of the University of Hokkeido; and the "Jules Verne Award" of the International Association for Hydrogen Energy.

Bass continued from page 18

the factors affecting reliability and to develop remedies therefrom, and to develop quantitative evidence of such a magnitude and consistent reproducibility as to ensure Goodstein's prophecy that the Establishment "will eventually be forced to take notice," will require allocation of resources of a magnitude which the Establishment presently reserves only for projects from which the ruling oligarchs perceive personal benefit—but, now that they have universally derided Cold Fusion as "pathological science," to reverse themselves would be as prestige-undermining as if an established religion had altered one of its doctrinally fundamental dogmas.

This is the great Catch-22 which is now hindering human progress: how often do biased judges voluntarily recuse themselves? (I recall the parable of the unjust judge [Luke 18:2-5] "who feared neither God nor man" but who eventually rendered justice to a persistent widow "lest by her continual coming she weary me.")

Those of us who are aware of the "evidence beyond a reasonable doubt" that Mizuno's new continent has been discovered need to petition our Congressional Representatives and our Senators until they "weary" of our persistence and force the DoE to "take notice" of the truth. May 1999 prove to be the year in which the truth becomes manifest to all!

New Energy Times Archive

Fred Jaeger

ENECO
391-B Chipeta Way
Salt Lake City, UT 84108 USA
Tel: 801-583-2000; Fax 801-583-6245
E-mail: jaeger@eneco-usa.com

Cold fusion was born in a controversy that has yet to subside. The dysfunctional nature of the dialogue has cast defenders into the untenable position of having to completely identify, up front, all reaction mechanisms and to provide totally reproducible evidence. Anything short of delivering these two points seems to allow critics to dismiss the entire field as some strange sort of pseudoscience. I can think of no other field of scientific exploration that has been held to such a high standard in the early stage. It fact, it defies the very definition of the word discovery.

At the ten year mark, I believe cold fusion still deserves to be examined as a legitimate topic for research. The technical challenges remain as formidable as ever, but the offsetting potential benefit to society is just too great to ignore. I am encouraged that many defenders have begun to lower their debating rhetoric so as not to present overly optimistic or unsupportable claims that inevitably erode credibility of the entire field.

At ENECO, we subscribe to the position of quietly seeking reproducible laboratory results utilizing very precise instrumentation and meticulous attention to details, especially with materials issues. Our general theoretical model of lattice-assisted energy transfers to nuclei guides all activities in our research plan. Confidence in our lab results comes only after countless hours of monitoring the operating characteristics of our system in order to readily distinguish between false-positive signals and actual energy excursions.

Unfortunately, our method of research still represents the norm at the ten year mark. It remains frustrating a decade later that only those persons who are willing or able to spend an inordinate amount of time and expertise on a very rigorous experimental setup are able to appreciate, firsthand, the subtlety of the cold fusion effect, even though it is perfectly normal in early stages of research to see only low-level inconsistencies in expected behavior. The current environment does not lend itself to rapid leveraging of ideas and expertise necessary for speedy development. Major breakthroughs will occur once we obtain a sufficient understanding of all the conditions necessary for reproducibility, scalability, and sustainability. Like all fields of research, a major breakthrough could occur tomorrow or in ten years. Success will primarily be a function of the number of well-designed and executed experiments being carried out on a global basis.

In this respect, it was gratifying to see nearly 200 scientists from twenty countries present their work at ICCF-7. It was especially rewarding to see several brave young scientists taking up the challenge and presenting papers in Vancouver for the very first time. The emerging generation of dedicated cold fusion researchers holds promise for the future, as the original pioneers begin to retire.

Where will the field be in ten years? I can not predict. There are just too many variables to ponder. Will the quest be worthwhile? I believe it will, not only for what marvelous secrets Mother Nature may reveal about cold fusion, but for all other useful discoveries that will inevitably be made in ancillary fields because a few determined persons chose to look very carefully at the unusual effects associated with a highly loaded lattice. We owe a tremendous collective gratitude to Drs. Pons and Fleischmann for allowing us to think in drastically new terms. The full history of this exciting journey is yet to be written. It is rewarding to be part of the process, and in some small way, to contribute to its ultimate success for the benefit of mankind.

The Author

FRED JAEGER, 52, received a B.S. degree in chemistry from the University of Cincinnati and an MBA from Harvard University Graduate School of Business. He is the president of ENECO, a privately held company in Salt Lake City that commercializes energy generation and conversion technology. Mr. Jaeger was the Organizational Chairman of the 7th International Conference on Cold Fusion (ICCF-7) held in Vancouver, Canada in April, 1998.

Fox continued from page 17

12. Although some thirty theories have been advanced to explain cold fusion, none of the theories have been embraced by the group of scientists studying low-energy nuclear reactions.

In summary, the discovery of cold fusion, although vigorously attacked (especially by hot fusion lobbyists), marked the beginning of a series of discoveries of low-energy nuclear reactions. Pons and Fleischmann deserve a Nobel Prize. The nuclear reactions are complex and not, as yet, fully explained. One or more of the subsequent developments of the various low-energy nuclear reaction investigations will result in commercial products. The energy needs of the world will, in a few years, be provided by new methods of energy production that do not depend on the burning of fossil fuels. How quickly these new energy developments will be commercialized and spread throughout the world will depend largely on the finding of financial resources and on a gradual acceptance by the scientific community of the reality of such discoveries.

This coming new century will not only benefit by energy production from sources now in the laboratory but will also benefit from similar new energy developments that will be used for on-site stabilization of radioactive wastes. Although much maligned, the discovery of cold nuclear fusion and ten years of development provides a strong beginning for a revolution in the understanding of low-energy nuclear reactions. The end results will be great future changes in the way we produce energy. This coming generation will be even more energy-rich than our current generation. The coming generation, however, will not be the planet polluters that this generation has been.

References

1. Debra R. Rolison, W.E. O'Grady, R.J. Doyle, Jr., and Patricia P. Trzaskoma (Naval Research Lab), "Anomalies in the Surface Analysis of Deuterated Palladium," Proceedings of the First Annual Conference on Cold Fusion, March 28-31, 1990, University of Utah Research Park, Salt Lake City, Utah.

2. Tadahiko Mizuno (Translated by Jed Rothwell), *Nuclear Transmutation: The Reality of Cold Fusion*, Infinity Energy Press, Concord, New Hampshire, c1997, 151 pages, illus., indexed.

New Energy Times Archive

COMMENTS ON THE TENTH ANNIVERSARY CONTRIBUTIONS

Jed Rothwell
Contributing Editor, Infinite Energy

We asked some of the leading cold fusion scientists to share their thoughts for this tenth anniversary issue. I was impressed by their responses, and touched by some of the deep emotions they expressed. I would like to focus on the political aspect of cold fusion. I think every contributor would agree with these points:

This discovery is on par with the discovery of nuclear fission by Hahn and Meitner in 1939 (Bockris).

The scientific establishment opposes us. "They will do everything possible to negate the funding because of the implication that their scientific judgment is not to be trusted" (Bockris).

The opposition is often irrational and closed minded. The dialogue between supporters and skeptics is dysfunctional and has "cast defenders into the untenable position of having to completely identify, up-front, all reaction mechanisms and to provide totally reproducible evidence" (Jaeger). Such demands are unprecedented in the history of science.

The reaction is much more difficult to reproduce than originally anticipated. There is a crying need for a reproducible reaction.

The inability to reproduce has been frustrating, but such inability is common with new discoveries, and should not be taken as evidence that the phenomenon does not exist. Nuclear bombs and tokamak plasma fusion reactors are much more difficult to reproduce than cold fusion, but no one claims they do not exist.

Many people think that a commercial demonstration would jolt the public and the scientific establishment into paying attention. Scott Chubb discounts this: "There is no simple cure for this problem: no single, smoking gun experiment or 'commercial demonstration' will convince the majority of scientists, or society, that cold fusion is real." Since a commercial demonstration has never been tried, there is no basis for saying it would not work. Historically, demonstrations of photography, telegraphs, the telephone, the x-ray, the airplane, microcomputer software and many other controversial inventions quickly won over the public.

This has been a "human drama" with "behind the scene conspiracies, scheming by vested interests, the mental block of the scientific establishment, the fear of ridicule by fellow scientists, the secrecy due to patent considerations" (Srinivasan). Srinivasan and Fleischmann believe there may be a conspiracy. Others, including Storms and myself, think we face overt obstinance, not a covert conspiracy.

Miley notes that early in the history of cold fusion there were statements that "any high school student can build and operate a cold fusion experiment," and this unreasonable optimism later became detrimental to the field and an embarrassment. This statement did circulate widely, and it should have embarrassed whoever said it. Pons, Fleischmann, Bockris, McKubre and others who understood cold fusion never said anything like this. They have always stressed the difficulties and unknown factors. This idea still circulates, so we should explain to the public that it is a misunderstanding.

Oriani writes, "Credibility of the field has been hurt by a large number of poor, uncritically done experiments and by a large body of poorly considered theoretical approaches." If this is a call for rigor, high standards, quality experiments and sufficient funding, I think everyone will agree. However, I hope Oriani is not suggesting that cold fusion researchers should be judged as a group, and that mistakes made by one researcher reflect badly upon another. Many self-styled "skeptics" condemn the field as a whole without making distinctions between researchers. McKubre cannot be held responsible for experiments performed by Mizuno or theories propounded by Conte.

Researchers have been consigned to an academic Siberia. It has been a long road, "tortuous, often dark and occasionally painful" (McKubre). Much of the blame for this fiasco rests on the establishment, but not all.

Beyond the Consensus

While I agree with this consensus, I would like to look beyond it. What can we do about these political problems? Suppose, for example, we cannot devise an easily reproducible experiment. Does that leave no way out? Cold fusion has enormous promise. Thousands of scientists would join in this research if only they knew the facts, and millions of people would invest in it. An easily reproducible experiment would be the quickest way to ignite widespread interest, but it cannot be the only way.

We feel that alternative strategies are essential. We should begin by paying more attention to the politics, obstructions, and human failings on our side. Perhaps the opposition is mainly to blame for this tragic fiasco; even so, we should concentrate on our own dysfunctional behavior and attitudes, for the simple reason that we cannot affect the opposition, but we can fix our own problems. The leading cold fusion scientists are among the world's most renowned electrochemists, including Martin Fleischmann, Fritz Will and John O'M. Bockris. After decades of being respected leaders in their fields and teaching brilliant graduate students, they do not suffer fools gladly. Over the years, technical disagreements have created certain hostile rivalries, even enmity. This has caused a great deal of damage to the field. We should look for common ground: points on which we can agree and unite.

We are stronger than we realize. We should boldly advertise our achievements. That has been the goal of this magazine from the start. Consider the reproducibility problem. Cold fusion remains difficult to reproduce, and most attempts fail. We hope that a simple recipe will someday allow nearly anyone to make a device that always produces a predictable level of heat. We have no idea when this might occur. Jaeger says, "A major breakthrough could occur tomorrow or in ten years." Suppose it does not? Then we must make better use of the breakthroughs that have already occurred, and the reproducibility already achieved. Consider the National Cold Fusion Institute (NCFI). This was established in 1989 by the state of Utah and endowed with $5 million. The director was Fritz Will, former president of the Electrochemical Society and head of General Electric's electrochemistry research. The Institute was staffed by an enthusiastic team of top-notch electrochemists. It closed in 1991. Little has been written about it in the popular press, and the researchers from NCFI published few peer-reviewed papers, because by the time they were ready to publish, most journals summarily rejected cold fusion papers. Researchers at NCFI proved that cold fusion produces tritium, a nuclear product.[1] Twenty different types of experiments were performed. Nine or ten of them produced elevated levels of neutrons or tritium. With the best method, developed in the later stages of

Dr. Fritz G. Will
Univ. of Utah

New Energy Times Archive

the project, four out of four heavy water experiments produced significant tritium, while none of the light water controls did. "Tritium enhancements up to a factor of fifty-two were observed." One hundred and fifty unused samples of palladium were tested extensively, by dissolving in acid and other methods. None of these samples had any measurable level of tritium.

NCFI work also showed that suspicions of contamination in other cold fusion experiments, at Texas A&M, were unfounded. Kevin Wolf initially reported tritium in two experiments out of several dozen. Later, he dissolved 125 palladium samples and exposed the solution to a liquid scintillator. He found evidence for tritium in three of the dissolved samples: two virgin samples and one that had been electrolyzed in light water. Based on this, Wolf retracted his initial results. However, Fritz Will said, "We duplicated his technique and found it to be open to all kinds of artifacts. As he applies it, is unsuitable to come up with any good results."[2]

So what is the recipe for replicating the NCFI results? It begins:

1. Get five million dollars.
2. Hire a team of the world's best electrochemists . . .

The team could save a lot of time and money by learning the techniques mastered by NCFI researchers and by diligently studying the detailed experimental data compiled by them. (One of the biggest problems in this field has been that few people read the literature carefully. The same mistakes are repeated over and over again.) After a year or so, the team could expect to see tritium in four out of four experiments. This is reproducibility, but it is not the kind we crave. We want a simple and cheap experiment that anyone can do. But this may be inherently impossible. After all, there is no simple, cheap way to reproduce a Pentium processor, the alloy used in a jet engine fan blade, or the Sistine Chapel. Many objects are inherently difficult to reproduce, including a surprising number that are widely used and vital to modern lifestyles. Microprocessors, laser printers, catalytic converters, long-life NiCad batteries and other high-tech products require expert attention, automated production lines, billion dollar factories, clean rooms, and precision and purity that would have seemed impossible a generation ago. Yet these difficulties have not prevented widespread use and continuing price reduction.

The NCFI tritium results should have been published on the front page of the *New York Times* and in every scientific journal and magazine on earth. Together with the replications by Bockris, Claytor and others, they should have convinced everyone that cold fusion is real, ending the debate. The $5 million in funding should have been increased to $500 million. The project was an unqualified success, yet no one believed it, no one would publish, and the Institute was forced to close. At ICCF-3 I spoke with Will. He was bitter and surprised. He is an influential scientist with credibility and decades of experience. I think he assumed that people would believe him and take him seriously. He showed the results to his colleagues, who must include many top decision-makers in industry and science. He sent reports to large corporations. He made a major effort to "shop around" for support, yet he failed. If a top scientist like Fritz Will brings stellar results like this to the corporate conference table, yet he cannot find support, what hope is there for the rest of us? He must have concluded there was no hope, because he left the field years ago. He did not retract his findings, but he gave up and returned to more conventional research. I told him I thought he should advertise his results, mount a public relations campaign, and do science by press conference. Instead of going through conventional channels to reach major corporations, we should try to attract small venture capitalists. Will said that is not how science is done. I agreed with him, but I pointed out that the opposition is not doing science the way it should be done. Following the rules is a sucker's game; we must try something else. We should learn from our arch rivals in hot fusion. They condemned Pons and Fleischmann for doing "science by press conference" and delaying the release of pre-prints by a few days, yet they call a triumphant press conference every time they fire up their machines, and they publish a paper six months later.[3]

Will, Fleischmann, McKubre and other leading researchers, who have the most convincing data, are part of the Establishment. They have successful track records doing things by the book and going through established corporate channels, so it may not have occurred to them to reach outside of these channels. They assumed their work would be treated like any other superbly done, important experiment. The industrial and scientific establishments are effective, after all. They do an admirable job of ensuring safety, economy, and efficiency with most incremental innovations like personal computers and the Internet. They are not as good at dealing with radical innovations.

Frankly, I think we must be more confrontational, like the activist organization ACT UP!, which has garnered widespread support for AIDS research. Oriani condemns "the polemical tone" of "some magazines." In conversation he added, "I hated to say that, but yes, I do mean your magazine." Fred Jaeger writes: "I am encouraged that many defenders have begun to lower their debating rhetoric so as not to present overly optimistic or unsupportable claims that inevitably erode credibility of the entire field." We disagree. Our confrontational stance at this magazine is deliberate. Oriani and Jaeger's approach may be intellectually honest in some sense, and it is the approved, traditional scientific style, but it is politically inept. We need to turn the rhetoric up and make outlandish promises. We must deal with unfair, cutthroat competition. In the computer and electronic industries, manufacturers promise the moon and customers automatically discount their claims. If you do not overstate your case, you are drowned out by noisy competition. In a rational academic debate, where the opposition played by the rules, Oriani would be right.

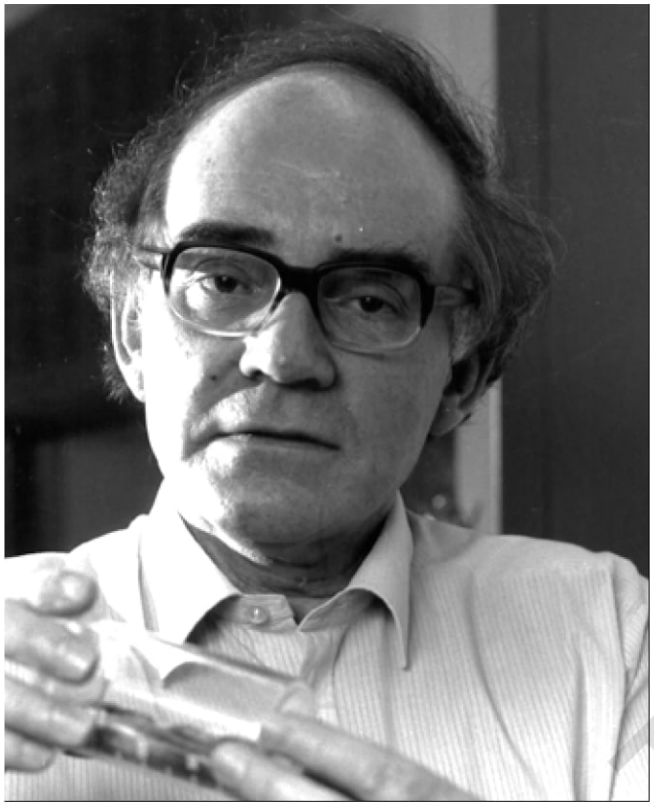
This is partly a cultural problem. Many scientists do not care for overt public relations. They understate their success and wrap positive assertions in layers of qualifications and escape clauses. The EPRI abstract says: "Rigorous attempts have been made to anticipate, exclude and define quantitative upper limits on artifacts and systematic error sources . . . None of the artifactual sources considered can account for the excess power and heat observed. It is therefore concluded, tentatively, that the source of excess energy is an unexpected, and as yet unexplained, property of the D/Pd system."[4] After years of intense labor and millions of dollars, this does not sound like a ringing endorsement. You would never guess that 300% excess at 90+ Sigma results were obtained. It sounds tepid and wishy-washy. This is the approved academic style, but a corporation writing product specifications in this kind of language would swiftly go out of business. Hot fusion lobbyists on Capitol Hill blow their own horns. They describe predictable, long-expected developments as exciting breakthroughs. A routine tokamak test run is shown on the seven o'clock news. If cold fusion scientists want to succeed in the larger public arena, they must learn the public idiom. I think they have no choice; they have been exiled from science. If they are

Rothwell continues on page 63

New Energy Times Archive

Nuclear Reactions in the Pd/D System: The Pre-History and History of Our Early Research

by Martin Fleischmann, FRS



Background

In the 1960's I became convinced that the behavior of ions in solution could only be explained in terms of Quantum Electrodynamics (Q.E.D.), rather than in terms of Classical Mechanics (C.M.), or Quantum Mechanics (Q.M.). I will not outline here the tortuous path which led to this conclusion; instead, I will use an illustration of the nature of the problem.

The generally accepted model of electrolyte solutions, the Debye-Hückel Theory, is based on the calculation of the energy of the systems due to the electrostatic interaction of the ions, Figure 1, *i.e.* on C.M. with some allowance for the fact that we are dealing with a many-body problem. In point of fact, the ions are not at rest and it has been known since the last century that ions move independently of each other at high dilutions (Kohlrausch). Moreover, the dominant motion of the ions is attributed to Brownian movements (Einstein) so that the ions must accelerate and decelerate, Figure 2. The concomitant radiation (Maxwell) should therefore cool the electrolyte solutions to absolute zero at which point the Debye-Hückel Theory would become valid. We therefore need to ask ourselves: why do the solutions not cool spontaneously? The answer is that the electrostatic model, when combined with Brownian motion, violates the Second Law of Thermodynamics (see Appendix A). In point of fact, the motion of the ions must be rigorously quantized and the correct theoretical framework is Q.E.D.

My attention returned at that time to the Pd/H and Pd/D systems. I had realized since the end of 1947 that these were the most extraordinary examples of electrolyte solutions. At that time I had found the early papers of Coehn, who had already shown in 1929 that H was present as H^+ in Pd host lattices (deuterium had not been discovered at that time). Moreover, the H^+ species was highly mobile in the lattice and, as the mobility obeyed the Nernst-Einstein relation, the species had to be present as "bare protons." In point of fact, the system behaves as an extremely dense plasma of protons (concentrations ~100 M) present within an ever higher concentration of electrons (~1,000 M).

The investigation of Coehn led to a number of very uncomfortable questions about the properties of the Pd/H and Pd/D systems. Evidently, the hydrogen ions have to be extremely strongly bound in the lattice so that the dissolution process can be exothermic (*i.e.* H^+ in the lattice is more strongly bound than in H_2). At the same time, the hydrogen ions are highly mobile, a conundrum which defied resolution within the framework of Q.M. (This is an example of the type of inconsistency referred to in Appendix A.) There was also the further question: would it be possible to change the potential energy of D^+ in the host lattice sufficiently (by means of applied electric fields) to induce nuclear reactions? My answer at that time was "no" (based on the available Q.M.) except possibly under "heroic conditions."

The matter rested there until the 1960's, when I came to realize that the Pd/H and Pd/D systems had to be modeled using Q.E.D. At that time, we started a number of haphazard investigations of the Pd/H system. The question of whether one could induce nuclear reactions became more clearly defined at the end of that decade. Work on the isotopic separation of H and D showed that it was necessary to assume that the H and D present had to be modeled as many-body systems in order to explain the macroscopic behavior. I assembled equipment to start work on the putative nuclear processes on two occasions, but each time decided that such research would be judged as being inconsistent with holding an Academic Appointment!

The matter rested there until 1983, at which time Stan Pons and I discussed a number of options for further research. We

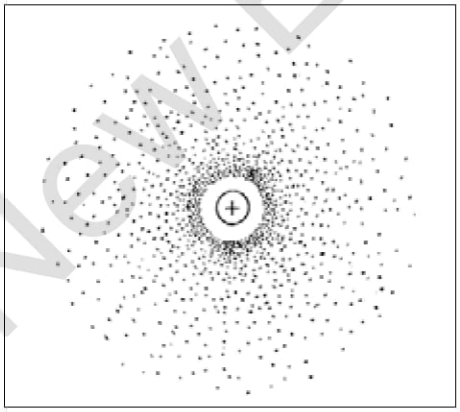


Figure 1. Schematic diagram of the negative space charge around a central positive ion formed by the electrostatic interactions of the ions.

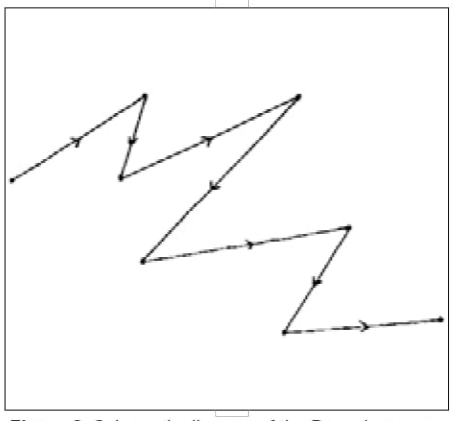


Figure 2. Schematic diagram of the Brownian movements of an ion. The arrows connect the points at which the ion is at rest.

New Energy Times Archive

believed that we could add one further major project to the topics then being investigated by our research groups. The topics we considered included:

(a) relativistic effects in chemical reactions;
(b) extension of the investigation of the structure and spec–troscopy of interfaces;
(c) the behavior of electrons in metals;
(d) nuclear reactions of D^+ in metal host lattices.

Of these projects, we decided that (a) was beyond our means; and (b) was dependent on obtaining major funding which we could not secure. Of the remaining two projects, (c) was our first choice, but it rapidly became apparent that this, too, was beyond our means.

One outcome of this research topic has been the demonstration that the majority of scientists are incapable of including serendipity in their thought processes.

We therefore embarked on (d) and considered the implications of carrying out this project. As I have already noted, I had previously excluded research work on this topic; however, I had by that time resigned from my full-time academic position and, therefore, felt free to tackle (d). At the same time, the situation facing my colleague would clearly be serious if the nature of this project ever became known. We decided that the project not only had to have a "hidden agenda," it had to be totally hidden. This was all the more necessary because the military applications of any positive outcome of the research were not at all clear.

Start-up of the Project

As is well-known, we posed the following two questions at the outset:

(1) Would the nuclear reactions of D^+ compressed into host lattices be different than the reactions in a dilute plasma (or than the reactions of highly excited D^+ in solids)?

(2) Could such changes in the reactions be observed?

We expected the answer to (1) to be "yes;" thus, as the D^+ in Pd-host lattices is present in a quantum system of macroscopic dimensions, the rates of reaction would inevitably be enhanced. However, we expected this enhancement to be insufficient to allow the observation of (2), so the answer to this question was likely to be "no." Nevertheless, we started a limited investigation and considered experiments based on the options A, B, C and E:

A: compression of D^+ in the lattices using electrodiffusion;

B: compression of D^+ in the lattices using electrochemical charging; the effect of plasma excitation on such systems or on A coupled to B;

C: charging of lattices by means of highly reducing/superbasic media;

D: highly oxidizing/superbasic media and the link to "Hot Fusion" (see below);

E: composite Systems, *e.g.* B or C linked to A.

Of these systems, A was our first choice, but we started with B (as a preliminary to A), because we believed that such systems are closest to the dictates of Q.E.D. (a view which was mistaken) and because electrochemical charging appeared to offer the easiest way of raising the potential energy of an extended Quantum System in an energy efficient way. Systems of type D were added to our list when it became clear that the nuclear reactions of D^+ in host lattices were radically different to those observed in "Hot Fusion" (see below).

As is also well-known, we opted for calorimetry as our primary "catch-all" methodology. Calorimetric methods can be made to be nearly as sensitive as the detection of nuclear particles and, indeed, are used in nuclear physics when it is necessary to make absolute measurements (*e.g.* in the estimation of Pu). Furthermore, the use of calorimetry was consistent with our wish to follow a "hidden agenda." The calorimetric method chosen had to meet a number of important criteria which included: conformation to "ideal behavior" (implying predictability of the performance from the Laws of Physics; we opted for the "well-stirred tank" familiar from the field of chemical engineering); high stability of the thermal impedances; uniformity of the temperature throughout the volumes of the cells, possibility of non-isothermal operation; high precision and accuracy and low unit cost—we were financing the projects personally. These criteria dictated the choice of isoperibolic calorimetry using modified Dewar vessels.

The Initial Results: Sauce for the Goose and Sauce for the Gander. Serendipity, the Path to March 1989

The overall structure of the problem had become reasonably clear by the summer of 1988. We were observing the generation of heat in excess of the enthalpy input to the cells and far above that commensurate with the generation of tritium and neutrons predicted by measurements on "Hot Fusion." Moreover, the excess enthalpy was far beyond that which could be attributed to any parasitic chemical reactions. It appeared, therefore, that it was possible to establish nuclear reactions in quantum systems of macroscopic dimensions (following the dictates of the Q.E.D. paradigm), which not only had much higher cross-sections than those predicted on the basis of two-body processes, but which also differed in kind from those observed for two-body reactions. Evidently, it would be necessary to establish the nature of the reaction path(s). The detection of the most likely product, ^4He, in the presence of a huge excess of D^+ would be a project of the utmost difficulty and quantitative correlations of the yield of ^4He with excess heat production even more so. (We obtained our first indications of the formation of ^4He at later dates, but considered these results to be unpublishable.)

This lack of "nuclear ash" has proved one of the main points of criticism of the many "skeptics." However, our initial consideration of this question led to a further series of problems best described by the epithet "what is sauce for the goose is sauce for the gander." Thus, to the best of our knowledge, the very low yield of ^4He in "Hot Fusion" (about 10^{-6} that of the yields of neutrons and tritium) appears to be based on the low yields of energetic γ-rays associated with the formation of ^4He in the two-body processes. While such a conclusion may be valid for nuclear reactions in dilute plasmas, it may be far from valid for "Hot Fusion" in solid phases (more especially for "Warm Fusion" in such systems). In fact, the work on "Hot Fusion" must be judged to be incomplete in the absence of precise thermal balances and correlations of the yields of ^4He with that of the production of γ-rays. It was therefore evident that the project would need to be extended to include:

D: Highly oxidizing/superacid media and the link to "Hot Fusion."

Our initial results (and the results obtained since 1988) appeared to indicate that there might be useful civilian applications without any military uses, a conclusion which ran counter to our initial expectations. However, closer reflection also showed that such a prediction might be incorrect. The system

New Energy Times Archive

we had developed was based on the premise of the Q.E.D. paradigm and it was not especially difficult to specify the changes which would need to be made to ensure that the system would converge onto the Q.M. paradigm — although we have never been questioned about this aspect and certainly have never had any intention of investigating the required changes.

In the summer of 1988 it had become clear that much further work was required, that the work would have to be broadened and that with an achievable acceleration of the program we might be able to assess the overall results by the autumn of 1990. We estimated that the cost of such an accelerated program would be ~ $600K which was above that which we could meet personally. At that time we also believed that we had reached a stage at which it was necessary to inform the United States Department of Energy and the United Kingdom Atomic Energy Authority of the nature of our research. This would in any event have become necessary at some stage during the research, but we decided that it was opportune to combine the information with an application for funds for the envisaged accelerated program. At that time we made it clear that we were unable to judge whether the work should be classified, at least for a defined period of time. We believed that we ourselves did not have the necessary information to reach a decision on this issue, but that, in any event, it was necessary to continue a secret program until such time as it would be possible to carry out a complete evaluation of the project. However, it transpired that the restrictions which we wanted to impose ran counter to the funding policies of the Department of Energy.

It is relevant to consider here the role of serendipity in this research (indeed, in any research). It has frequently been asserted that these discoveries were made "by serendipity." This view is incorrect although serendipity certainly played a part in the progress of the work. In my view, the true role of serendipity is the recognition of the significance of unusual results. It is better to guide one's research by a series of logical steps rather than to indulge in a process best described as "Gee Whiz." However, it is also important to accept and explain unusual results rather than to ascribe them to unspecified errors. One outcome of this research topic has been the demonstration that the majority of scientists are incapable of including serendipity in their thought processes.

The Position in March 1989

The outcome of our application to the Department of Energy can best be described as leading to the "worst case scenario" for our future research work: the whole matter was evidently going to be forced into the public domain. We therefore had to disclose our results to the administrative authorities of the University of Utah, who, in turn, felt bound to apply for Patent Protection. It is important to recall that we had by that time reached specific rates of energy production roughly equal to those in gas cooled fission reactors. In turn, the patent applications became the driving force for future actions.

The fact that this scenario followed this disastrous course has frequently been criticized. However, one must ask: "Can one imagine that the events could have followed any other scenario once the research was going to be driven into the public domain?"

I believe that it is important here to outline some of our reasons for wishing to delay the publication of the results. In the first place, although we had indications for the formation of ^{4}He, these results were not publishable; secondly, we believed that most scientists would judge the work on the basis of the Q.M. paradigm and would therefore conclude that the results had to be false; thirdly, we did not believe that industry would conclude that research in this field (let alone any products based on this research!) would be in their short or medium term interest, although there might well be initial flashes of enthusiasm; fourthly, we believed that those concerned with national security could hardly be expected to welcome such research in the university sector; fifthly, we believed that we would lose our freedom of action because the research would become constrained by targets ill-matched to achieving further progress; lastly, we really wished to return to the more general problem of searching for examples of the operation of the Q.E.D. paradigm.

Post March 1989

As was to be expected, the topic of the nuclear reactions of D^+ in host lattices has turned out to be much more complicated and extensive than was apparent in March 1989. These are matters which will be described in other articles contributed to this issue of *Infinite Energy*. Furthermore, it has become apparent that the systems are "pseudo-simple": while they are relatively simple to set up, their behavior is actually very complicated. Most of the key variables are "hidden" and are difficult to evaluate because the systems are only subject to very few controllable parameters (cell current or voltage, temperature for any given material).

As far as the generation of excess enthalpy using scenario B is concerned, it has become apparent that the production of low levels of excess enthalpy is easy to demonstrate in the early stages of the experiments provided the experiments are carried out with high precision and accuracy and provided satisfactory electrode materials are used. These provisos have turned out to be critically important, problems which have not always been understood. With increasing time and/or temperature, the systems pass through a region of "positive feedback" in the sense that increases in temperature lead to increases in the rates of excess enthalpy production. This "positive feedback" greatly complicates the investigation, another matter which has not been generally understood. "Positive feedback" leads to much higher levels in the rates of excess enthalpy generation, including the sustained production of heat at the boiling points at rates roughly equivalent to those achieved in fast breeder fission reactors. Furthermore, it has become apparent that it is possible to construct a number of systems operating above and beyond the onset of "positive feedback" and which generate excess enthalpy at lower levels but at zero enthalpy input. These phenomena have been described with a number of epithets including "Heat-after-Death," "Heat-after-Life" and "After-Effects." The work of Mengoli *et al.* on this topic is especially noteworthy.[1]

The use of devices based on "Heat-after-Death," etc. appears to open up the route to a range of "niche applications." The work on systems of type B appears to open up the way to a much wider range of applications, especially to those which require the utilization of low-grade heat. However, it has been shown that much higher specific rates of excess enthalpy production (in the range 10 - 100 kW cm^{-3}) can be achieved using systems of type A (note especially the work of Preparata and Del Giudice).[2] Systems of this type may well lead to a very wide range of applications.

Appendix A. Choosing the Correct Paradigm: The Influence of Paradigms on Scientific Research

The example I have used in the main text (the use of electrostatics in the Debye-Hückel Theory) is a useful illustration of the influence of paradigms on scientific research. We understand the world in terms of models which are based in turn on

New Energy Times Archive

particular paradigms. I believe that all scientists would agree that, in constructing such models, we should use the simplest possible paradigm sufficient for the task at hand. Thus, the Debye-Hückel Theory leads to the interpretation of the variation of the self-energy of electrolyte solutions with the concentration. We do not need to use a more complicated paradigm as long as our attention is limited to the thermodynamics of such systems (albeit at very low concentrations).

At the same time, there are dangers in such a minimalist approach. The paradigm (and the associated models) come to dominate our thinking so that it becomes difficult (perhaps even impossible) to ask whether deviations from any predicted behavior may not be due to the use of an invalid paradigm? We do not ask the question: what would be the consequences of using a different paradigm? Instead, we attempt "to save the paradigm," with increasingly improbable special assumptions. Furthermore, the range of application of the paradigm is frequently extended to the interpretation of properties (or other properties of other systems) where its use is restrictive (possibly even incorrect). Thus, for the illustration used in this article, electrostatic models have been used in the interpretation of the dynamical properties of electrolyte solutions. Inevitably, such interpretations are kinematic: the consequences of using dynamic models remain hidden from view.

Evidently, we need some methodologies for choosing appropriate paradigms, a problem which has been much discussed in the philosophy, history and sociology of science. Such discussions usually center on the question: Were there (or are there) an increasing range of phenomena which could not (or cannot) be explained by an existing paradigm? We should also discuss the question: Can we arrive at "better interpretations" of given phenomena by changing the paradigm? However, this second question could open the way for much musing of doubtful value. It has, therefore, always seemed to me that we should use more "hard-nosed" approaches and I have found two such approaches to be especially useful.

In the first of these approaches, we can ask the questions: Can a given property A be interpreted in terms of paradigm X by using the argument (a)? Can a second property B be interpreted in terms of paradigm X by using the argument (b)? Such a list can be extended to include further properties. We then ask the further question: Are (a) and (b) self-consistent? If this is not the case, then the cause is likely to be the inapplicability of paradigm X. It is of interest that the dissolution of hydrogen and deuterium in palladium affords several examples of such inconsistencies if the paradigm X is Q.M.

In the second approach, we investigate whether the application of the paradigm leads to violations of the Second Law of Thermodynamics. I believe that most scientists would agree that the inability to violate this law (or else of some related principle such as microscopic reversibility) is our first line of defense against the development of invalid interpretations. However, it is also clear that whereas scientists are very adept at criticizing engineers, members of the public, or even each other (!) for the invention of devices based on Perpetual Motion, they are not very adept to subject paradigms and their derived models to the same scrutiny. The main text contains an illustration of this principle which led to the conclusion that processes in condensed matter must be interpreted in terms of Q.E.D.

Appendix B. The Search for Demonstrations and the Need to Invoke the Q.E.D. Paradigm

In the 1960's it appeared to me that it should be possible to devise experiments which would demonstrate the need to model the behavior of condensed phases in terms of Q.E.D. rather than in terms of C.M. or Q.M. At that time (and at various times since then) I discussed some of the problems involved with other scientists whom I placed in two broad categories: those familiar with Q.E.D. and those, who, as far as I could judge, were not familiar with this particular field. Such a classification was indeed useful: the response of those in the first group to the problems raised by the modeling of electrolyte solutions was always immediate: "You must use Q.E.D."

The response of those in the second group was uniformly negativeand, frequently, quite violently so. I came to believe that any research on this topic would have to follow a "a hidden agenda." The topics had to be chosen so that they could be justified in terms of foreseeable interpretations in terms of C.M. or Q.M; the specification of Q.E.D. at the outset would simply lead to sterile debate which would frustrate the research. The need to invoke this paradigm would, therefore, have to emerge from the interpretation of parts of the problem which were not essential to those parts leading to interpretations based on C.M. or Q.M., *i.e* it was necessary to be able to say "given that the systems behave in the way seen, we can then use C.M. or Q.M. to reach the following conclusions." Q.E.D. would be restricted to the interpretation of the statement: "Given that the systems behave in the way seen."

The particular approach, which I favored at that time, was the study of the dynamics of very small systems as revealed by the direct observation of the fluctuations of the properties. (This will explain in part my interest in the study of electrodes having very small dimension, the so-called "microelectrode.") In the fullness of time we could specify four systems which had the potential of satisfying the "hidden agenda." Two of these systems were investigated but research on one of these had to be abandoned because of the pressure of work on "Cold Fusion;" evaluation of the data sets for the other system was not completed for the self same reason (specifically, these aspects which required interpretation by Q.E.D.). In addition, there were nine further projects, of which four were investigated. However, the interpretation of the results for these systems were (or were expected to be) dependent on deviations from the predictions based on C.M or Q.M. The demonstration of the applicability of the Q.E.D. paradigm was, therefore, not clear cut. It became evident that this line of investigation suffered from a major drawback in that the present day theory of stochastic processes is quasi-classical because the effects of Q.M. are only included to the extent of the recognition of distinct quantum states. Progress with the topics was, therefore, dependent on the development of the theory of stochastic processes based on Q.E.D.

Our attention was therefore directed to systems where the applicability of the Q.E.D. paradigm would become apparent from more macroscopic observations. (An approach which had actually preceded that based on the measurement of fluctuations in small systems and which led to the realization that electrolyte solutions have to be modeled using this paradigm. This aspect will not be described here.) The most extraordinary of these projects was based on the two questions:

(1) Would the nuclear reactions of D^+ compressed into host lattices be different from the reactions in a dilute plasma (or from the reactions of highly excited D^+ in solids)?

(2) Could such changes in the reactions be observed?

References

1. G. Mengoli *et al.* 1998. *J. Electronal. Chem.*, 441, p. 155.

2. Giuliano Preparata. 1996. *Proceedings of ICCF-6*, p. 136.

New Energy Times Archive

Key Experiments that Substantiate Cold Fusion Phenomena

Compiled by Eugene F. Mallove, Sc.D., Editor-in-Chief, *Infinite Energy*
P.O. Box 2816, Concord, NH 03302-2816 www.infinite-energy.com

Several thousand technical papers now exist in the area that has become known as "cold fusion" and "low-energy nuclear reactions" (LENR), or "chemically assisted nuclear reactions" (CANR). The papers that are highlighted here are those that might be considered the "Select 34" publications that report *experimental results*. The salient conclusions of each study are abstracted. These experimental results, taken from a much larger body, make what we consider to be an irrefutable case for the reality of cold fusion phenomena. Note well: This selection is *not* intended to detract from the very high value of many, many other quality papers that have been published in the field. Furthermore, this selection does not include the large number of papers that discuss the *theoretical* foundations of cold fusion phenomena. (The order of paper presentation is somewhat arbitrary. It is designed to give a feel for the kinds of results and the variety of institutions reporting them.)

Los Alamos National Laboratory
Nuclear Materials Technology Division

"Electrolytic Tritium Production," Edmund Storms and Carol Talcott, *Fusion Technology*, Vol. 17, July 1990, pp. 680-695.

Fifty-three electrolytic cells of various configurations and electrode compositions were examined for tritium production. Significant tritium was found in eleven cells at levels between 1.5 and 80 times the starting concentration after enrichment corrections are made.

Los Alamos National Laboratory
Nuclear Materials Technology Division

"Review of Experimental Observations About the Cold Fusion Effect," Edmund Storms, *Fusion Technology*, Vol. 20, December 1991, pp. 433-477.

The experimental literature describing the cold fusion phenomenon is reviewed. The number and variety of careful experimental measurements of heat, tritium, neutron, and helium production strongly support the occurrence of nuclear reactions in a metal lattice near room temperature, as proposed by Pons and Fleischmann, and independently by Jones.

SRI International *and* Electric Power Research Institute

"Development of Advanced Concepts for Nuclear Processes in Deuterated Metals," M.C.H. McKubre, *et al.*, EPRI TR-104195, Research Project 3170-01, Final Report, August 1994, 128 pages, plus 342 pages on microfiche.

This work confirms the claims of Fleischmann, Pons, and Hawkins of the production of excess heat in deuterium-loaded palladium cathodes at levels too large for chemical transformation. . . Although nuclear reaction products commensurate with the excess heat have not yet been observed, small but definite evidence of nuclear reactions have been detected at levels some forty orders of magnitude greater than predicted by conventional nuclear theory.

SRI International *and* Electric Power Research Institute

"Isothermal Flow Calorimetric Investigations of the D/Pd and H/Pd Systems," M.C.H. McKubre, S. Crouch-Baker, R.C. Rocha-Filho, S.I. Smedley, F.L. Tanzella, T.O. Passell, and J. Santucci, *Journal of Electroanaytical Chemistry*, **368**, 1994, pp. 55-66.

The generation of "excess power" was observed in a series of deuterium-based experiments, but not in a hydrogen-based experiment. The results of these experiments enable several (tentative) conclusions to be reached concerning the conditions necessary for the reproducible observation of this anomalous thermal effect.

Naval Air Warfare Center Weapons Division,
Research Department, Chemistry Division *and*
University of Texas, Department of Chemistry

"Anomalous Effects Involving Excess Power, Radiation, and Helium Production During D_2O Electrolysis Using Palladium Cathodes," Melvin H. Miles, Benjamin F. Bush, and Joseph J. Lagowski, *Fusion Technology*, Vol. 25, July 1994, pp. 478-486.

Previous experiments showed that eight electrolysis gas samples collected during episodes of excess power production in two identical cells contained measurable amounts of ^4He while six control samples gave no evidence for helium... This places the ^4He production rate at 10^{11} to 10^{12} atom/s per watt of excess power, which is the correct magnitude for typical fusion reactions that yield helium as a product... Simultaneous evidence for excess power, helium production, and anomalous radiation was present in these experiments. Completely new experiments with more precise helium measurements are reported that again show simultaneous evidence for excess power, helium production, and anomalous radiation.

Naval Ocean Systems Center *and*
U.S. Department of Energy (Washington)

"On the Behavior of Pd Deposited in the Presence of Evolving Deuterium," S. Szpak (Navy), J.J. Smith (DoE), *J. Electroanalytical Chemistry*, **302** (March 11, 1991), pp. 255-260.

Three sets of preliminary experimental results are presented here, *i.e.*, the production of excess enthalpy, the production of tritium, and the presence of some form of radiation.

Texas A&M University

"Triggering of Heat and Sub-surface Changes in Pd-D Systems," J. O'M. Bockris, R. Sundaresan, Z. Minevski, and D. Letts, Transactions of *Fusion Technology* (Proceedings of ICCF-4), Vol. 26, 4T, Part 2, December 1994, p. 267-290.

More than four years after the first reports of chemically stimulated nuclear reactions, the triggering of heat evolution and the production of associated nuclear debris is still a highly uncertain matter. This is so both as to the duration of the switch-on time and, indeed, whether a given electrode will commence to show nuclear activity within 500 hours of the beginning of electrolysis. In the present study, three methods of triggering anomalous heat are described; the changes in the sub-surface of palladium during the evolution of D_2 or H_2 are described as a function of potential, temperature, and time. Finally, these results are brought up against the present theories of heat production in metals.

Texas A&M University

"Tritium and Helium Production in Palladium Electrodes and the Fugacity of Deuterium Therein," John O'M. Bockris, Chun-

New Energy Times Archive

Ching Chien, Dalibor Hodko, Zoran Minevski, *Frontiers of Cold Fusion, Proceedings of the Third International Conference on Cold Fusion (Nagoya, Japan 21-25 October, 1992)*, edited by Dr. Hideo Ikegami, National Institute for Fusion Science, Nagoya 464-01, Japan, pp. 231-240.

An account is given of the massive production of tritium at a Pd electrode. Production continued for ~750 hours after which time it was arbitrarily curtailed. Production of T was found to cease every few days but could be resuscitated by increasing the overpotential of the electrode reaction. A logarithmic relation between the rate of tritium production and the overpotential of the electrode reaction was established. The Will-Cedzynska method of examining T contamination in specimens has shown that nothing above the background of T was detected if no D_2O had been electrolytically evolved on the Pd specimens concerned. Helium production was found to accompany that of T. The He was analyzed by thermal expulsion and mass spectroscopy. No He^3 was found but He^4 was measured in nine specimens out of ten examined. Voids were also detected ~1 micron within the electrode. The excess tritium production on Pd co-deposited with deuterium was found. . .

University of Minnesota

"Calorimetric Measurements of Excess Power Output During the Cathodic Charging of Deuterium into Palladium," R.A. Oriani, J. C. Nelson, S. Lee, and J. H. Broadhurst, *Fusion Technol.*, 18 (1990) pp. 652-658.

A Seebeck-effect calorimeter was used to establish that generation of energy, in excess of the electrical energy input, can occur during the electrolysis of D_2O. The magnitude of the excess power is measured with respect to the electrolysis of H_2O as the baseline. The excess power levels of >60 W/cm^3 palladium and excess energies of 74 kJ cannot be understood in terms of recombination of D_2 and O_2 within the calorimeter, other chemical reactions, or a storage-and-relaxation mechanism.

University of Utah and University of Southampton

"Electrochemically Induced Nuclear Fusion of Deuterium," Martin Fleischmann and Stanley Pons, *Journal of Electroanalytical Chemistry*, 261, 1989, pp. 301-308; Errata, *Journal of Electroanalytical Chemistry*, 263 , 1989, pp. 187-189.

Enthalpy generation can exceed 10 W cm^{-3} of the palladium electrode; this is maintained for experiment times in excess of 120 h, during which typically heat in excess of 4 MJ cm^{-3} of electrode volume was liberated. It is inconceivable that this could be due to anything but nuclear processes.

University of Utah

"Calorimetry of the Palladium-Deuterium-Heavy Water System," Martin Fleischmann, Stanley Pons, Mark W. Anderson, Lian Jun Li, Marvin Hawkins, *J. Electroanalytical Chemistry*, 287, No.2, 25 July 1990, pp.293-348.

It is shown that accurate values of the rates of enthalpy generation in the electrolysis of light and heavy water can be obtained from measurements in simple, single compartment Dewar type calorimeter cells. This precise evaluation of the rate of enthalpy generation relies on the non-linear regression fitting of the "black-box" model of the calorimeter to an extensive set of temperature time measurements. The method of data analysis gives a systematic underestimate of the enthalpy output and, in consequence, a slightly negative excess rate of enthalpy generation for an extensive set of blank experiments using both light and heavy water. By contrast, the electrolysis of heavy water at palladium electrodes shows a positive excess rate of enthalpy generation; this rate increases markedly with current density, reaching values of approximately 100 W cm^{-3} at approximately 1 A cm^{-2}. It is also shown that prolonged polarization of palladium cathodes in heavy water leads to bursts in the rate of enthalpy generation: the thermal output of the cells exceeds the enthalpy input (or the total energy input) to the cells by factors in excess of 40 during these bursts. The total specific energy output during the bursts as well as the total specific energy output of fully charged electrodes subjected to prolonged polarization (5-50 MJ cm^{-3}) is 10^2-10^3 times larger than the enthalpy of reaction of chemical processes.

IMRA Europe *and* University of Southampton

"Calorimetry of the Pd-D_2O System: From Simplicity Via Complications to Simplicity," M. Fleischmann and S. Pons, *Physics Letters A*, 176 (1993), pp. 118-129.

Abstract: We present here one aspect of our recent research on the calorimetry of the Pd/D_2O system which has been concerned with high rates of specific excess enthalpy generation (> 1 kW cm^{-3}) at temperatures close to (or at) the boiling point of the electrolyte solution. This has led to a particularly simple method of deriving the rate of excess enthalpy production based on measuring the times required to boil the cells to dryness, this process being followed by using time-lapse video recordings. Our use of this simple method as well as our investigations of the results of other research groups prompts us to present also other simple methods of data analysis which we have used in the preliminary evaluations of these systems.

Mills Technologies *and* Ursinus College

"Excess Heat Production by the Electrolysis of an Aqueous Potassium Carbonate Electrolyte and the Implications for Cold Fusion," Randell L. Mills and Steven O. Kneizys, *Fusion Technology*, Vol. 20, August 1991, pp. 65-81.

Calorimetry of pulsed current and continuous electrolysis of aqueous potassium carbonate (K^+/K^+ electrocatalytic couple) at a nickel cathode is performed in single cell dewar calorimetry cells. Excess power out exceeded input power by a factor of >37. **[Note:** Further spectacular results through 1999 are to be found on the company's web site: www.blacklightpower.com; see also Mike Carrell's article in this issue page 36.]

National Cold Fusion Institute

"Reproducible Tritium Generation in Electrochemical Cells Employing Palladium Cathodes with High Deuterium Loading," Fritz G. Will, Krystyna Cedzynska, and Denton C. Linton, *Journal of Electroanalytical Chem.*, **360**, 1993, pp. 161-176

Reproducible tritium generation well above background has been observed in tightly closed D_2SO_4 containing cells in four out of four Pd wire cathodes of one type. . . The largest amount of tritium, generated in seven days of continuous electrolysis, was 2.1 x 10^{11} tritium atoms, compared with a background of 4 x 10^9 tritium atoms. . . It is concluded that the tritium was generated inside the Pd; only nuclear reactions, whose nature is as yet unknown, could have produced the observed tritium.

New Energy Times Archive

Naval Air Warfare Center Weapons Division

"Anomalous Effects in Deuterated Systems," by Melvin H. Miles, Benjamin F. Bush, and Kendall B. Johnson, NAWCWPNS Technical Publication 8302, September 1996, 99 pages.

Excess power was measured in 28 out of 94 electrochemical experiments conducted using palladium or palladium-alloy cathodes in heavy water. . .Results from our laboratory indicate that helium-4 is the missing nuclear product accompanying the excess heat. Thirty out of 33 experiments showed a correlation between either excess power and helium production or no excess power and no excess helium. The collection of the electrolysis gases in both glass and metal flasks place the helium-4 production rate at 10^{11} to 10^{12} atoms per second per watt of excess power. This is the correct magnitude for typical deuteron fusion reactions that yield helium-4 as a product. Anomalous radiation was detected in some experiments by the use of X-ray films, Geiger-Mueller counters, and by the use of sodium iodide detectors. There was never any significant production of tritium in any of our experiments. . . Our results provide compelling evidence that the anomalous effects in deuterated systems are real...It is highly unlikely that our heat and helium correlations could be due to random errors. . . Our best experiments produced up to 30% excess heat, 0.52 watts of excess power, and 1400 kilojoules (kJ) of excess enthalpy. This amount of excess enthalpy is difficult to explain by any chemical reaction. . . Anomalous radiation was detected in some experiments by the use of X-ray films, several different types of Geiger-Mueller (GM) counters, and sodium iodide (NaI) detectors. Normal radiation counts were always observed when no electrolysis experiments were running. . . [Note: This entire report was reprinted in Infinite Energy No. 15/16 pp. 35-59.]

Scientific Industrial Association LUTCH (Russia)

"Nuclear Product Ratio for Glow Discharge in Deuterium," A.B. Karabut, Ya.R. Kucherov, and I.B. Savvatimova, *Physics Letters A*, 170 (1992), pp. 265-272.

New results for glow discharge in deuterium calorimetry are presented. In separate experiments a heat output five times exceeding the input electric power was observed. The result for the charged particle spectrum measurement is presented. Charged particles with energies up to 18 MeV and an average energy of 2-4 MeV were seen. Beams of gamma-rays with energies of about 200 keV and a characteristic X-ray radiation were registered. The summed energy of the registered products is three orders short of the values needed to explain the calorimetric results.

Shell Research S.A. (France)

"Cold Fusion by Sparking in Hydrogen Isotopes," Jacques Dufour, *Fusion Technology*, Vol. 24, September 1993, pp. 205-228.

Excess energy production, well above the background and in amounts of the same order of magnitude as the input energy, has been measured. This is caused by sparking in hydrogen isotopes between electrodes made of metallic hydride-forming metals (palladium and stainless steel). This excess energy production is stable over long periods (several weeks) and is observed with both hydrogen and deuterium. Only extremely low levels of neutrons and tritium have been detected, many orders of magnitude below what would be expected from the excess energy production measured. On the contrary, copious emission of low-energy radiation (likely to be beta rays) has been observed. A class of hypothetical nuclear reactions, based on the action of the weak electronuclear force, is proposed that accounts for all the experimental facts observed.

NASA Lewis Research Center

"Replication of the Apparent Excess Heat Effect in a Light Water-Potassium Carbonate-Nickel Electrolytic Cell," Janis M. Niedra, Ira T. Meyers, Gustave C. Fralick, and Richard S. Baldwin, NASA Technical Memorandum 107167, February 1996.

Replication of experiments claiming to demonstrate excess heat production in light water-Ni-K_2CO_3 electrolytic cells was found to produce an apparent excess heat of 11 watts maximum for 60 W electrical power into the cell. Power gains ranged from 1.06 to 1.68. . .

Portland State University Department of Physics

"Surface Morphology and Microcomposition of Palladium Cathodes After Electrolysis in Acidified Light and Heavy Water: Correlation with Excess Heat," J. Dash, G. Noble and D. Diman, Transactions of *Fusion Technology* (Proceedings of ICCF-4), Vol. 26, 4T, Part 2, December 1994, 299-306.

Experiments were performed using Pt anodes and Pd cathodes. The electrolyte contained H_2O and H_2SO_4 in one cell and D_2O and H_2SO_4 in a similar cell connected in series. Excess heat, localized melting, and localized concentrations of Au or Ag were observed. The most probable explanation for these phenomena is nuclear fusion.

Los Alamos National Laboratory

"Tritium Production from a Low Voltage Deuterium Discharge on Palladium and Other Metals," T.N. Claytor, D.D. Jackson, and D.G. Tuggle, published on WWW and reprinted in *Infinite Energy*, No. 7, March-April 1996, pp. 39-42,

Over the past year we have been able to demonstrate that a plasma loading method produces an exciting and unexpected amount of tritium from small palladium wires. In contrast to electrochemical hydrogen or deuterium loading of palladium, this method yields a reproducible tritium generation rate when various electrical and physical conditions are met. . . We will show tritium generation rates for deuterium-palladium foreground runs that are up to twenty-five times larger than hydrogen-palladium control experiments using materials from the same batch. [See also, "Tritium Evolution from Various Morphologies of Deuterated Palladium," *Proceedings of the Fourth International Conference on Cold Fusion*, December 6-9, 1993, Maui, Hawaii, Edited by Y.O. Passell, EPRI TR-104188, July 1994.]

University of Rome, Department of Chemistry *and* Laboratory of Physics, Istituto Superiore de Sanita

"X-ray, Heat Excess, and ^{4}He in the Electrochemical Confinement of Deuterium in Palladium," F. Cellucci, P.L. Cignini, G. Gigli, D. Gozzi, M. Tomellini, E. Cisbani, S. Frullani, F. Garibaldi, M. Jodice, G.M. Urciuoli, *Progress in New Hydrogen Energy*, Vol. 1 and Vol. 2, *Proceedings of the Sixth International Conference on Cold Fusion* (ICCF-6), October 13-18, 1996, pp. 3-11.

The energy balance between heat excess an ^{4}He in the gas phase has been found reasonably satisfied even if the low levels of ^{4}He found do not give the necessary confidence to state definitely that we are dealing with the fusion of deuterons to give ^{4}He.

Osaka University

New Energy Times Archive

"Solid State Plasma Fusion ('Cold Fusion')," Yoshiaki Arata and Yue-Chang Zhang, *Journal of the High Temperature Society of Japan*, Vol.23, January 1997, 56 pages (the entire Volume!)

Using a quadrupole mass spectrometer ("QMS"), significantly large amounts of helium. . .were detected from deuterated palladium samples that had produced large amounts of anomalous excess energy (200-500 MJ/cm^3) during Cold Fusion experiments (~5,000 [hr]). The helium was released from samples after the observation of excess energy, and only when each sample was heated to a high temperature (>1,000 °C) in a high vacuum (~3 x 10^{-9} [Torr]).

Fusion Studies Laboratory, University of Illinois *and* Clean Energy Technologies, Inc.

"Nuclear Transmutations in Thin-Film Nickel Coatings Undergoing Electrolysis," George H. Miley and James A. Patterson, paper delivered at the Second International Conference on Low Energy Nuclear Reactions, Texas A&M University, September 13-14, 1996, published also in *Infinite Energy*, No. 9, 1996, pp. 19-32.

Experiments using 1-mm plastic and glass microspheres coated with single and multilayers of thin films of various metals such as palladium and nickel, used in a packed-bed electrolytic cell (Patterson Power Cell™ Configuration), have apparently produced a variety of nuclear reaction products. The analysis of a run with 650-Å film of Ni is presented here. Following a two-week electrolytic run, the Ni film was found to contain Fe, Ag, Cu, Mg, and Cr, in concentrations exceeding 2 atom % each, plus a number of additional trace elements. These elements were at the most, only present in the initial film and the electrolyte plus other accessible cell components in much smaller amounts. That fact, combined with other data, such as deviations from natural isotope abundances, seemingly eliminates the alternate explanation of impurities concentrating in the film. Results showing a broad array of products, such as found here, have also been obtained with thin film coatings of other materials, e.g., Pd and multi-layers of Pd and Ni. The yields of the major elements contributing depend on the film material, however. . . The array of products found in these experiments is consistent with recent studies of solid Pd and Au electrodes by Mizuno *et al.*, 1996 and Ohmori and Enyo, 1996, respectively . . .

Mitsubishi Heavy Industries, Ltd.
Advanced Technology Research Center

"Detection of Anomalous Elements, X-ray and Excess Heat Induced by Continuous Diffusion of Deuterium Through Multilayer Cathode (Pd/CaO/Pd)," Yasuhiro Iwamura, Takehiko Itoh, Nobuaki Gotoh, Mitsuru Sakano, Ichiro Toyoda, and Hiroshi Sakata, *Proceedings of the Seventh International Conference on Cold Fusion* (ICCF-7), April 19-24, 1998, pp. 167-171, also published in *Infinite Energy*, No. 20, 1998, pp. 56-58.

A new type of experimental apparatus is developed to induce nuclear reactions by continuous diffusion of deuterium. Ti atoms, which cannot be explained by contamination, were detected on the surface where deuterium atoms passed through on Pd cathodes after electrolysis. A multilayer cathode (Pd/CaO/Pd) is introduced based on an EINR (Electron Induced Nuclear Reaction) model. Excess heat generations and x-ray emissions were observed for all the cases we tried by the multi-layer cathodes. $^{57}Fe/^{56}Fe$ ratio of Fe atoms detected in the multilayer cathodes is anomalously larger than natural $^{57}Fe/^{56}Fe$.

University of Lecce, Department of Physics (Italy)
Laboratory of Applied Electronics

"Morphologic Deformation and Distribution of Generated Elements in Saturated Palladium Samples Processed by a UV Excimer Laser," Vincenzo Nassisi, *Infinite Energy*, No. 15/16, July-November 1997, pp. 119-122.

This work reports on the experimental results obtained from saturated palladium hydrides processed by a XeCl excimer laser. This process provided a screening effect of 14.2 when it was applied to deuterated palladium samples. They presented a low neutron emission and a strong tendency to release particles when were perturbed by O_2. The samples processed showed morphologic changes with formation of pits and superficial stresses. Besides, the transmutation of many elements, Al, Au, C, Ca, Cl, Fe, K, Mg, Na, Nd, Ni, O, S, Si, V and Zn, was recorded.

Hokkaido University

"Detection of Radiation Emission, Heat Generation, and Elements from a Pt Electrode Induced by Electrolytic Discharge in Alkaline Solutions," Tadahiko Mizuno, Tadayoshi Ohmori, and Tadashi Akimoto, *Proceedings of the Seventh International Conference on Cold Fusion* (ICCF-7), April 19-24, 1998, pp. 253-258.

Radiation emission, heat generation, and detection of elements were confirmed during electrolytic discharge at high voltage in a light water solution containing an alkaline ion from the platinum electrode.

Departement de Physique
Faculte des Sciences de Luminy (France)

"Excess Heat Measurement in $AlLaO_3$ Doped with Deuterium," Jean-Paul Biberian, *Proceedings of the Fifth International Conference on Cold Fusion* (ICCF-5), April 9-13, 1995, Monte Carlo, Monaco, pp. 49-56.

We show evidence that solid state electrolytes can be used successfully in "cold fusion" experiments. We describe in this work that $LaAlO_3$ single crystals loaded with deuterium produce excess heat up to ten times the amount of electrical power applied. No significant amount of neutrons has been detected.

University of Minnesota, Corrosion Research Center
Department of Chemical Engineering and Materials Science

"An Investigation of Anomalous Thermal Power Generation from A Proton-Conducting Oxide," R.A. Oriani, *Fusion Technology*, Vol. 30, Nov. 1996, pp. 281-287.

A high temperature Seebeck-effect calorimeter, in which the thermoelectric electromotive force across a large-area-enveloping thermopile is a measure of the heat flux from a power source, has been constructed to examine the claimed generation of excess thermal energy from a proton-conducting oxide immersed in deuterium gas. The claim has been confirmed in a few experiments out of many unsuccessful ones.

Tsinghua University, Department of Physics (China)

"'Excess Heat' Measurement in Gas-loading D/Pd System," Xing Zhong Li, Wei Zhong Yue, Gui Song Huang,. Hang Shi, Lan Gao, Meng Lin Liu, Feng Shan Bu, *Progress in New Hydrogen Energy*, Vol. 1 and Vol. 2, *Proceedings of the Sixth International Conference on Cold Fusion* (ICCF-6), October 13-18, 1996, Toya, Japan, pp. 455-462.

A gas-loading D/Pd system has been designed to measure

New Energy Times Archive

the "excess heat." The preliminary result has shown that the calorimetric feature of the D/Pd system is distinct from that of its twin H/Pd system. The difference between these twin systems can be attributed to the "excess heat" of the order of watts per cubic centimeter of palladium. . . We have observed this excess heat for more than 5 months. . . It is about 1,000 eV for each palladium atom, which is very difficult to be attributed to any chemical resource.

Bhabha Atomic Research Centre (India)

"Reproducible, Anomalous Emissions from Palladium Deuteride/Hydride," R.K. Rout, A. Shyam, M. Srinivasan, and A.B. Garg, V. K. Shrikhande, *Fusion Technology*, Vol. 30, November 1996, pp. 273-280.

Each and every palladium sample loaded/reloaded either with hydrogen or deuterium was observed to fog radiographic films kept in its close proximity in air. Strangely, even with ten layers of black paper (thickness ~63 mg/cm^2) as a filter between film and sample, fogging was observed. On the other hand, no fogging could be observed even when thin beryllium foil (~ 1.4 mg/cm^2), three layers of transparent polyester foils (~10 mg/cm^2), or thin aluminized polycarbonate (0.3 mg/cm^2) were employed as filters. Several experiments have been performed to identify the phenomenon responsible for fogging. These experiments appear to rule out any of the known mechanisms, suggesting a new, strange, and unknown phenomena.

Philips Research Laboratories (The Netherlands)

"Reduced Radioactivity of Tritium in Small Titanium Particles," Otto Reifenschweiler, *Physics Letters A*, 184 (1994), pp. 149-153.

By heating a $TiT_{0.0035}$ preparation consisting of extremely small monocrystalline particles (ϕ ~15 nm) a decrease of the radioactivity by 40% was observed. In further experiments the concentration of tritium in such preparations was varied (TiT_x experiments) showing that the radioactivity of the tritium increased less than proportionally to its concentration. Careful analysis of the experiments seems to rule out the possibility of trivial errors. A provisional hypothetical explanation is formulated. Our experiments may point to a connection with cold DD-fusion.

First Gate Energies, Mountain View, California

"Predictable and Reproducible Heat," Roger Stringham, John Chandler, Russ George, Tom Passell, and Richard Raymond, *Proceedings of the Seventh International Conference on Cold Fusion* (ICCF-7), April 19-24, 1998, Vancouver, British Columbia, Canada, pp. 361-365.

The application of transient cavitation bubbles, TCBs, in conjunction with metal foils has proved to be a good scientific probe or tool for the predictable measurements of excess heat Q(x) and heat-after Q(a) at steady state temperatures. The TCB jet produces a high density coherent flux of D^+ that is injected into the foil lattice with a flux density of about 10^{24} D^+/cm^2 in a few pico seconds and at 40 KHz billions of TCBs are formed on the lattice each second. The data is measured at 5 min. intervals over a period of 24 hours in a static device with some 43 sonication runs and an equal number of calibration runs. A Joule heater is included in the piezo-driven reactor to calibrate all runs. It was found from earlier unpublished work that a number of foils will produce both Q(x) and Q(a) in D_2O. The Q(a) is different in a mixture of light and heavy water. Q(a) is divided into two distinct processes; Q(al) involving just deuterons and the larger Q(a2) involving both deuterons and protons. The knowledge gained from 43 experiments reported on in this paper were completed in the period of February to April of 1998. The energy values for Q(x) and Q(a') outputs vary from 40 KJ to mega Joules with rates that vary from 1 to 15 watts depending on the conditions. The one parameter that is not predictable at this point is the duration of Q(a)

JET Energy Technology, Inc. (Wellesley, MA) *and* Massachusetts Institute of Technology

"Optimal Operating Point Characteristics of Nickel light Water Experiments," Mitchell R. Swartz, *Proceedings of the Seventh International Conference on Cold Fusion* (ICCF-7), April 19-24, 1998, Vancouver, British Columbia, Canada, pp. 371-377.

Nickel light-water electrochemically loaded systems with a variety of cathodes have been investigated using low-noise multiring calorimetry. Plots of excess heat, or power gain, as a function of input electric power drive indicate a narrow locus of optimal system operating points. At the center of the optimal operating point, the peak power ratio or power gain may be at a relative maximum. Driving with electrical input power beyond this operating point yields a typical falloff of the observed power ratio for increasing input power or current levels toward a power gain ratio of 1 and less. This optimal operating point behavior to increasing input electrical power drive may be general behavior for both palladium-heavy water and nickel-light water systems including the production of ash and enthalpy.

National Institute for Nuclear Physics (INFN-LNF) *and* CISE and ORIM (Italy)

"Preliminary Results with 'Cincinnati Group Cell' on Thorium 'Transmutation' Under 50 Hz AC Excitation," F. Celani, M. Achilli, A. Battaglia, C. Cattaneo, C. Buzzanca, P.G. Sona, A. Mancini, *Proceedings of the Seventh International Conference on Cold Fusion* (ICCF-7), April 19-24, 1998, Vancouver, British Columbia, Canada, pp. 56-67.

We give the procedure and the results of experiments performed with a standard "Cincinnati Group Cell," aimed to observe possible "transmutation" of Th in other elements via an AC electrolytic process. Three techniques have been used to avoid bias due to spurious effects: α-radiometry, ICP/MS and ICP/optical, looking at difference between initial and final solution and between blank (no Th) and "black (with Th) processed solutions. We found a deficit of Th after processing and new elements produced. The results are still not conclusive on transmutation and we discuss arguments in favor and against the transmutation hypothesis. We clarify the critical points of the measurement techniques, adding some suggestions to improve the reliability of results in future measurements.

Key Cold Fusion Compendia and Reviews

• Dr. Edmund Storms (Los Alamos National Laboratory), "Review of Experimental Observations About the Cold Fusion Effect," *Fusion Technology*, 1991, Vol. 20, December, 1991, pp. 433-477.

• Dr. M. Srinivasan (Bhabha Atomic Research Centre, Bombay, India), "Nuclear Fusion in an Atomic Lattice: Update on the

New Energy Times Archive

International Status of Cold Fusion Research," *Current Science*, April 25, 1991.
• "A Review of the Investigations of the Fleischmann-Pons Phenomena," John O'M. Bockris, Guang H. Lin, and Nigle J.C. Packham, *Fusion Technology*, Vol. 18, August 1990, pp. 11-31.
• *BARC Studies in Cold Fusion* (April-September 1989), Bhabha Atomic Research Centre, BARC - 1500, December, 1989, P.K. Iyengar and M. Srinivasan; also in *Fusion Technology*, Vol. 18, August 1990, pp. 32-94.
• *First Annual Conference on Cold Fusion (March 28-31, 1990): Conference Proceedings*, by the National Cold Fusion Institute, Salt Lake City.
• *Anomalous Nuclear Effects in Deuterium/Solid Systems*, American Institute of Physics Conference Proceedings 228, 1991, Steven E. Jones, Franco Scaramuzzi, and David Worledge (editors), Proceedings of an International Progress Review on Anomalous Nuclear Effects in Deuterium/Solid Systems, Brigham Young University, Provo, Utah, October 22-24, 1990. (approx. 1000 pages)
• *Investigation of Cold Fusion Phenomena in Deuterated Metals* (four volumes), by the National Cold Fusion Institute (Salt Lake City), June 1991, now available from NTIS.
• *The Science of Cold Fusion: Proceedings of the Second Annual Conference on Cold Fusion*, June 29-July 4, 1991, Como, Italy, published by the Italian Physical Society, Bologna, Italy, 1991, edited by T. Bressani, E. Del Giudice, and G. Preparata (528 pages).
• *Frontiers of Cold Fusion, Proceedings of the Third International Conference on Cold Fusion* (Nagoya, Japan 21-25 October, 1992), edited by Dr. Hideo Ikegami, National Institute for Fusion Science, Nagoya 464-01, Japan.
• *Proceedings: Fourth International Conference on Cold Fusion* (December 6-9, 1993, Lahaina, Maui, Hawaii), published by the Electric Power Research Institute (EPRI), the conference was sponsored by EPRI and the Office of Naval Research. Four volumes of the proceedings edited by Thomas O. Passell (EPRI). A large selection of papers from the Proceedings were peer-reviewed and published in a dedicated issue of *Fusion Technology*, December 1994, Volume 26, No.4T, part 2, 535 pages.
•*Proceedings of the Fifth International Conference on Cold Fusion* (ICCF-5), April 9-13, 1995, Monte Carlo, Monaco, 640 pages.
•*Progress in New Hydrogen Energy*, Vol. 1 and Vol. 2, *Proceedings of the Sixth International Conference on Cold Fusion* (ICCF-6), October 13-18, 1996,Toya, Japan, 709 pages.
• *Proceedings of the Seventh International Conference on Cold Fusion* (ICCF-7), April 19-24, 1998, Vancouver, British Columbia, Canada, Published by ENECO, Salt Lake City, 512 pages.
• *Cold Fusion Source Book: International Symposium on Cold Fusion and Advanced Energy Sources*, Belarusian State University, Minsk, Belarus, May 24-26, 1994. Edited by Hal Fox, published by Fusion Information Center, Salt Lake City.
• *A Critical Review of the "Cold Fusion Effect,"* Edmund Storms, December 1994, *Journal of Scientific Exploration*, Vol. 10, No. 2, 1996, pp. 185-243.
• "Cold Fusion Research in Italy," F. Scaramuzzi, *Proc. Third International Conference on Cold Fusion*, October 21-25, 1992, Nagoya Japan, *Frontiers of Cold Fusion*, (H. Ikegami, ed), p. 353.
• "Cold Fusion Researches in Russia," Tsarev, V., *Proc. Third International Conference on Cold Fusion*, October 21-25, 1992, Nagoya Japan, *Frontiers of Cold Fusion*, (H. Ikegami, ed), p. 341. And, "Cold Fusion Studies in the USSR," Tsarev, V., *Proc. of the Second Annual Conference on Cold Fusion*, Como, Italy, June 29—July 4, 1991, *The Science of Cold Fusion*, Vol. 33 (T. Bressani, E. Del Giudice and G. Preparata, eds), p. 319.
• "Chinese Effort in Understanding the "Cold Fusion" Phenomena," Li, X. Z., *Proc. of the Second Annual Conference on Cold Fusion*, Como, Italy, June 29-July 4, 1991, *The Science of Cold Fusion*, Vol. 33 (T. Bressani, E. Del Giudice and G. Preparata, eds), p. 309. And: "Cold Fusion Researches in China-From Confirmation to Analyzing the Mechanism," Li, X. Z., *Proc. Third International Conference on Cold Fusion*, October 21-25, 1992, Nagoya Japan, *Frontiers of Cold Fusion*, (H. Ikegami, ed), p. 337. Zhong, L. X., "Searching for Truth With High Expectations- 5 Year Studies on Cold Fusion in China,"*Proc. Fourth International Conference on Cold Fusion*, Lahaina, Maui, Dec. 6-9, 1993.

"Negative" Experiments That Were Not Negative

More than any other experiments, the so-called "null" experimental results of three prestigious laboratories in 1989—Caltech, Harwell Laboratory (UK), and the MIT Plasma Fusion Center—gave wide currency to the idea that cold fusion was "dead" and that other laboratories (of supposedly lesser repute) were obtaining lower-quality results that, although positive, were likely in error. Though this notion was convenient for the hot fusion researchers and other academics who mocked cold fusion from the outset, this idea is based on a totally false premise. The very "null" results of this triumvirate of major labs are in serious doubt—and this is an understatement.

The calorimetry work of each and every one of the major null- result reporting labs—Caltech, Harwell Laboratory, and MIT Plasma Fusion Center— have all been critiqued and found to be seriously flawed technically. This has not prevented the U.S. Department of Energy, the U.S. Patent Office (USPTO), and the general news media from continuing to recite endlessly that in 1989 "almost no one got positive results except a handful of laboratories."

To clarify the record on the common historical fallacy that was so central to the history of cold fusion for the past decade, we list here some of the formally published critiques that condemn the results from Caltech, Harwell, and MIT:

• Swartz, Dr. Mitchell R., "Re-Examination of a Key Cold Fusion Experiment: 'Phase-II' Calorimetry by the MIT Plasma Fusion Center," *Fusion Facts*, August 1992, pp. 27-40.
• Swartz, Dr. Mitchell R., "A Method to Improve Algorithms Used to Detect Steady State Excess Enthalpy,"*Proceedings: Fourth International Conference on Cold Fusion* (December 6-9, 1993, Lahaina, Maui, Hawaii), and in Transactions of *Fusion Technology*, Vol. 26, December 1994, pp. 369-372.
• Swartz, Dr. Mitchell R., "Some Lessons from Optical Examination of the PFC Phase-II Calorimetric Curves, *Proceedings: Fourth International Conference on Cold Fusion* (December 6-9, 1993, Lahaina, Maui, Hawaii).
• Noninski, Drs. V.C. and C.I. "Comments on 'Measurement and Analysis of Neutron and Gamma Ray Emission Rates, Other Fusion Products, and Power, in Electrochemical Cells Having Palladium Cathodes,'" *Fusion Technology*, Vol. 19, May 1991, pp. 579-580.
• Miles, Melvin H., B.F. Bush, and D. Stillwell, "Calorimetric Principles and Problems in Measurements of Excess Power During Pd-D_2O Electrolysis," *J. Physical Chemistry*, Feb. 17, 1994, pp. 1948-1952.
• Hansen, Wilford N. and M.E. Melich, "Pd/D Calorimetry—The Key to the F/P Effect and a Challenge to Science,"*Proceedings: Fourth International Conference on Cold Fusion* (December 6-9, 1993, Lahaina, Maui, Hawaii), and in Transactions of *Fusion Technology*, Vol. 26, December 1994, pp. 355-368.

❐❐❐

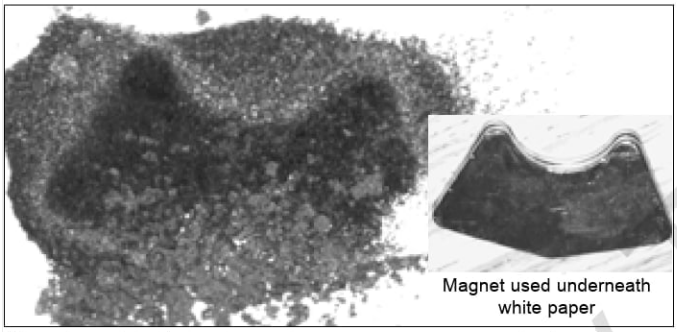
New Energy Times Archive

DEVICE AND PROCESS TESTING UPDATES

Conducted by Ed Wall and Eugene Mallove

Work on the Ohmori-Mizuno plasma electrolysis exploration cell sent to us for testing by NRG, (Nova Resources Group), (see *IE* No. 23) has continued. We have focused on the "detritus" produced—the substance found at the bottom of the cell after running. It is collected by filtration and is found to have properties that could loosely be described as "ferromagnetic." When thoroughly dried and spread out on paper, a magnet under the paper will affect the pattern that the substance forms, much like iron filings will do, only not as strongly (see photo below). Perhaps this is not ferromagnetism but paramagnetism. Whatever the explanation, there is no doubt that the effect is fully repeatable. Powdered "virgin" pure carbon rod (Alfa-Aesar, nominally < 5 ppm Fe) used for the electrodes will not do this.

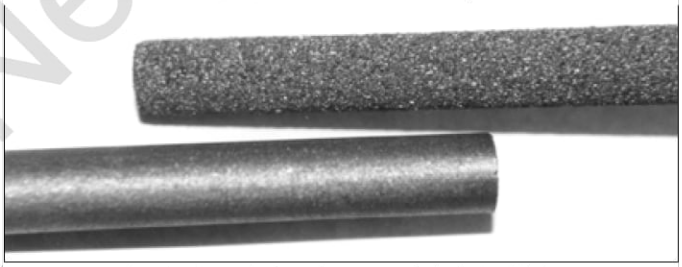
We had the detritus tested for eight metals by an outside testing laboratory (Granite State Analytical, Inc. of Derry, New Hampshire). The results from *two* test series indicated a weak presence of iron (0.60 mg/l in the filtrate, 0.01% and 0.03% by weight in the detritus, 0.0% and 0.02% by weight in the virgin carbon rod). Noteworthy in our second test series is a strong presence of calcium, both in the detritus and the filtrate, but not in the rod—for which we know of no conventional source (other than possible impurity in the K_2CO_3 electrolyte, which has not yet been tested). We are certainly mindful that a few cold fusion researchers claim to have found transmutation of K to Ca. See the second series results tabulated below (Table 1). The problems with possible contamination are foremost on our minds, so this work is only preliminary, except for the magnetically active detritus, for which we are seeking a conventional explanation. Colleague Michael J. Schaffer commented to us: "Many materials are magnetic. Ferromagnetism requires unpaired electrons plus sufficient coupling to align all the spins of neighboring atoms in the same direction. The former is a common property and gives paramagnetism, a relatively weak, but measurable magnetic response. Spin alignment is a much rarer phenomenon and is what turns weak paramagnetism into strong ferromagnetism. . . You might just have a magnetic compound of K and C and O."



When thoroughly dried and spread out on paper, a magnet under the paper causes bristling in the detritus. *Photo by Ed Wall*

We are also studying the energy balance of the cell by examining the steam condensate that is collected and comparing that with input electrical energy. An inherent uncertainty exists in measuring power into an arcing load, because the spectrum of frequencies is extremely broad. The best method would be to use a high sampling rate digital oscilloscope, which can multiply voltage and current (voltage collected across a shunt resistor) at each sampling interval. Unfortunately, we do not yet have such an instrument. However, we used our Amprobe DM-II Power Analyzer on the AC (60 Hz) power input to the rectifier that provides the DC power to drive the cell. Harmonic analysis of the oscillographs of that sinusoid reveals a very minor portion of the power contained in harmonics that would not be measured accurately by this device. Even if input power is measured accurately, there are losses in the rectifier. Also, heat and intense light radiated from the cell and plumbing are not reflected in the quantity of condensate collected—nor is the energy of any hydrogen gas that may be produced accounted for. That is why the NRG-developed cell comes with a heater for

Cell before use. Note much larger anode than cathode area. Attached silver coin was used for neutron detection (none found). *Photo by Ed Wall.*



Anode tip cp. virgin carbon rod. *Photo by Ed Wall.*

Device Updates continued on page 57

Table 1. Second Series of Test Results (Earlier Series was not as complete.)

	Detritus Sample #6, 1.6031 grams	Alfa-Aesar Carbon Rod #2 1.0203 grams
Iron	0.03 %	0.02 %
Chromium	0.00 %	0.00 %
Calcium	0.47 %	0.16 %
Potassium	5.76 %	0.62 %
Aluminum	0.10 %	0.09 %
Magnesium	0.08 %	0.04 %
Copper	0.00 %	0.00 %
Manganese	0.00 %	0.00 %

Liquid Filtrate Analysis (approx. volume, 500 ml)

	Concentration		Total Amount		% of 2.5gm of Exposed Cathode	
Iron	0.60	mg/l	0.30	mg	0.012	%
Chromium	0.11	mg/l	0.055	mg	0.0022	%
Calcium	229.	mg/l	114.5	mg	9.16	%
Aluminum	4.23	mg/l	2.115	mg	0.085	%
Magnesium	3.5	mg/l	1.75	mg	0.070	%
Copper	0.09	mg/l	0.045	mg	0.0018	%
Manganese	0.03	mg/l	0.015	mg	0.0006	%

New Energy Times Archive

EMERGING BLACKLIGHT POWER:
Synopsis and Commentary

Mike Carrell
mikec@snip.net

In January 1999, BlackLight Power Inc. (BLP) announced the discovery of a new family of chemicals with extraordinary properties, and moved their corporate headquarters to East Windsor, New Jersey. East Windsor is near Princeton University, an area with an impressive cluster of high technology laboratories. BLP's new facility was originally built by RCA AstroElectronics for satellite production and testing in 1958. With the purchase of RCA by GE and following mergers into Lockheed-Martin, satellite production was moved elsewhere and the facility was abandoned. Also nearby is the Forrestal Laboratory of Princeton University, site of the U.S. Tokomak project. It faces a bleak future, as Congress has cut funds for the failed hot fusion program.

BLP intends this facility to be a licensed laboratory with a staff of about 100 Ph.D.'s to support development of the power and chemical aspects of BLP, one of the very promising energy technologies of the twenty-first century. The full story of BLP is available at their Website, <http://www.blacklightpower.com>. This site is uncommonly complete, with hundreds of pages of background, test data and theoretical discussion. Some of it requires the Adobe Acrobat reader, which is a free download, available by a click on the BLP pages.

The term BlackLight Power comes from the extreme ultraviolet radiation which occurs in the BLP process. In the BLP process, hydrogen atoms are catalyzed by proximity to ionized potassium atoms, which absorb energy from the hydrogen atom and induce the electron orbit to shrink, yielding energy greater than conventional chemistry, but less than nuclear levels. The shrunken hydrogen atoms are called *hydrinos*. If an electron source is present in the reaction, *hydrino hydrides* can form, which are chemically active and can form the basis of a wide range of novel chemical compounds.

Origins of BLP Technology

In 1986 Randell Mills, M.D. was a graduate student at MIT in electrical engineering. In an inspired moment, he realized that the hydrogen atom and its orbiting electron could be represented in a novel way, an *orbitsphere*, which allowed the application of classical electromagnetism to the calculation of the emission lines of hydrogen from first principles. His theory also predicted that the orbit of the hydrogen electron can be induced to shrink to a lower energy state, with the release of large amounts of energy. The process is catalytic, requiring close proximity to other atoms that could absorb the released energy and remove it. These atoms must present a matching, resonant "energy hole" to absorb the released energy. Otherwise the reaction does not occur.

Randell Mills, founder of BlackLight Power. *From BlackLight Website.*

When the Fleischmann and Pons story broke in 1989, Mills realized that an electrolytic cell might provide an environment in which to test his theory. Mills searched for elements and ionization states which might provide the predicted energy hole. The most feasible was potassium. Mills built a cell with a nickel cathode and electrolyte of potassium carbonate in ordinary water. The cell produced excess heat without the long loading period then experienced in the deuterated cathodes of those following the Fleischmann and Pons system.

In the confusing reports in the "cold fusion" field at the time, it was not widely realized that the Mills cell is fundamentally different in the energy release process at work since the energy release is associated with a reduction in the electron orbit, not a change in a nucleus. Thus the Mills BLP process is a form of super chemistry, outside the realm of nuclear physics or "cold fusion."

Mills founded Hydrocatalysis Corporation near Lancaster, Pennsylvania. He entered into a contract with Thermacore to build and test cells based on his theories. He also subcontracted work to Penn State University and to Lehigh University for measurements of reaction products in his cells, particularly evidence for hydrinos. He has followed a pattern of subcontracting work to selected university and commercial laboratories to good advantage. The cost of recruiting people and establishing a laboratory was avoided. Further, since the work was done by independent laboratories, their reports gave a growing credibility to the theoretical projections of Mills' theory.

Mills' theoretical base is delineated in his *Grand Unified Theory of Classical Quantum Mechanics*, a book of 557 pages, with a new, expanded edition due in February. In the GUT, he extends his calculations of emission lines from the BLP process and hydrinos to match known emissions from the Sun and deep space which had not been matched to known physical processes. The BLP Web site contains reproductions of essential theoretical and experimental data from his book.

Gas Phase BLP

While the $H_20 + K_2CO_3$ cells with nickel cathodes produced robust excess heat, and could operate above boiling with pressurization, the power density was not suitable for scale-up to serve the major utility industry. In 1995, he built a small cell to test a gas phase reaction between hydrogen atoms and ionized

New Energy Times Archive

potassium as a catalyst. His theory predicted that the energy released in the reaction would appear as extreme ultraviolet radiation, so he changed the name of the company to BlackLight Power. It is intended that BlackLight will become a widely recognized trade name through press releases in late 1999 and products in the twenty-first century.

On the basis of the gas phase experimental results, in 1997 BLP raised $10.6 million in private placements, in which several large utilities participated.

BLP Commercial Strategy

Beyond the private placements, BLP expects to proceed by licensing its technology into various markets. In power generation, for example, there could be licenses for several ranges of power output, each with its particular market niche. For the chemicals, there could again be a range of identified chemicals and target markets. BLP can accept applications and then auction each segment with an up-front fee and continuing royalties. In this way, funds for continuing development may be obtained. Issuance of public stock remains an option, depending on later developments.

A key element in BLP strategy is a rechargeable battery technology based on hydrino hydride compounds. BLP projects that batteries with a specific energy of 600,000+ Watt-Hours/Kg. can be realized. The impact of such technology on the transportation, power generation and distribution markets is discussed in the Website.

The license route distributes risk and allocates to specialized companies the task of development for markets best known to the licensees. For this, BLP will need the high caliber staff they intend to hire, and will need the license fees and royalties to support such a staff in the Princeton area.

The BLP Process

Below is a graphical representation of the BLP process, which is most correctly expressed in the equations given on the Website and in Mills' book (Figure 1). The energy released is many times that required to produce atomic hydrogen from water. Thus, BLP power generators use water as a "fuel" and release oxygen to the atmosphere.

Experiments and Validation

Physical theories are tested by comparing the numerical results of experiments and observations with calculations from first principles using the theory in question. Where two theories can each account for an experimental result, the theory which encompasses the widest range of observational data with the fewest assumptions is usually accepted. Mills' theory is remarkable, for he claims it to be valid over 45 orders of magnitude, from the nuclear to cosmic scale.

Observations of the sun and cosmos have shown radiation lines which have not been assigned to any known reaction or process. Mills' theory predicts many of the deep-space and solar radiation lines. If the sun were wholly powered by known nuclear reactions, there should be a neutrino flux much greater than has been observed. The energy release of the sun may be principally the BLP process, with nuclear processes playing a secondary role.

A theory which leads directly to a new experiment, which works immediately and robustly, deserves close attention. Such is the case with the electrolytic cell built in 1990. Mills predicted hydrogen collapse as a source of energy and found potassium carbonate as the most promising catalyst (others are mentioned in Mills' Australian patent—reprinted in *Infinite Energy* No. 17, pp. 67-72). It gave excess heat immediately, without the long loading period characteristic of the palladium-heavy water system disclosed by Fleischmann and Pons.

Later, electrolytic cells were built whose cathodes were hollow, made of thin nickel, with the interior evacuated and connected to analytical instruments. Hydrinos formed at the cathode diffused through the nickel cathode and were detected by gas chromatography. There are many other experiments showing the existence of hydrinos and hydrino hydrides on the BLP Website.

Mills has published a few papers on his work in peer-reviewed journals. Much of the experimental work has been in other laboratories, some under subcontract, with information shared with specific agencies, corporations, and shareholders. Because of the proprietary value of the technology, there was little motivation to expose details until basic patent protection was in place. The business strategy of targeting large utilities as a primary market meant that the audience for the experimental results was a handful of high-level engineers and managers.

BLP then opened its Website, with a major update in January 1999. It contains over 200 pages of detailed reports of tests of BLP cells and new compounds formed in the cells. Apparently taken from patent applications, the reports are detailed with respect to the laboratories used, the method of sample preparation, the analytical instruments used, and the test results. There is too much to reproduce here, but a sample will illustrate the character of the material.

Hydrino Hydride Compounds

Hydrinos are electrically neutral. If an electron source is available to the reaction, hydrino hydrides form which can combine with other elements to form novel compounds.

Look at the plot (see Figure 2) of the binding energy (energy necessary to liberate an electron by X-ray bombardment) from a glassy carbon rod which had been the cathode of electrolytic cell with 0.57 Mole potassium carbonate electrolyte for three months. The bumps in the curve are associated with binding energies calculated for hydrinos in various stages of collapse.

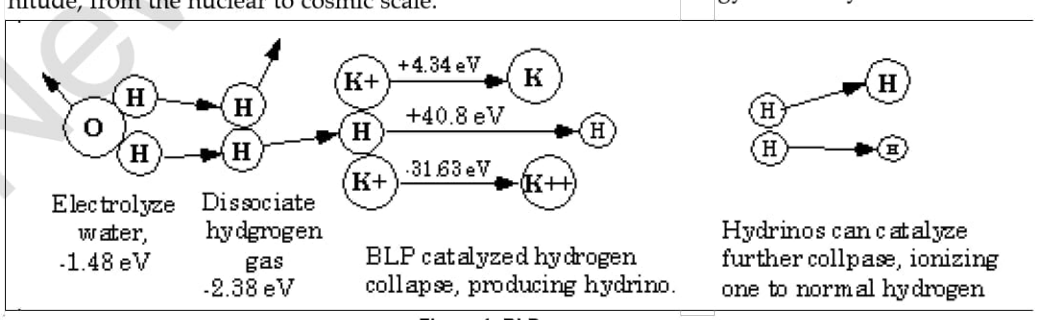


Figure 1. BLP process.

New Energy Times Archive

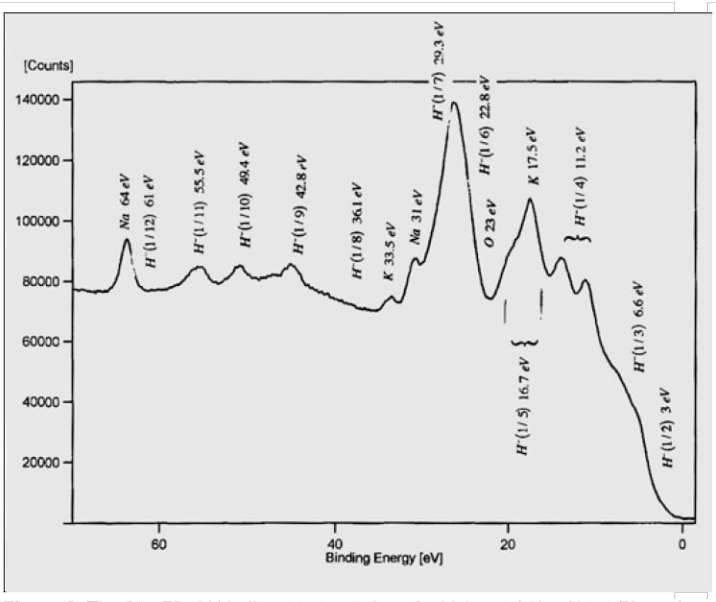


Figure 2. The 0 to 70 eV binding energy region of a high resolution X-ray Photoelectron Spectrum (XPS) of a glassy carbon rod cathode following electroysis of a 0.57M K_2CO_3 electrolyte and storage for three months (sample #3).

The laboratories and techniques used in this work include:

Laboratory	Analytical Test Performed
Lehigh University (XPS)	X-ray Photoelectron Spectroscopy
Virginia Tech	Raman Spectroscopy
Charles Evans & Associates East	TOFSIMS, XPS, EDS, Scanning Electron Spectroscopy
Charles Evans & Associates West	TOFSIMS
Xerox	TOFSIMS, XPS
Physical Electronics, Inc.	TOFSIMS
Spectral Data Services	Proton & K NMR
Surface Science Associates	FTIR
IC Laboratories	XRD
Ricerca, Inc.	LC/MS
PerSeptive Biosystems	ESITOFMS
INP	EUV Spectroscopy
Galbraith Laboratories	Elemental Analysis
Franklin & Marshall College	XRD
Pennsylvania State University	Calvet Calorimetry, XRD
TA Instruments	TGA/DTA
Northeastern University	Mossbauer Spectroscopy
M-Scan inc.	FABMSMS, ESIMS, Solids Probe Magnetic Sector Mass Spectroscopy
Micromass	ESITOFMS
Southwest Research Institute	Solids Probe and Direct Exposure Probe Magnetic Sector Mass Spectroscopy
BlackLight Power	EUV Spectroscopy, Cryopgenic Gas Chromatography, Thermal Decomposition/ Gas Chromatography, Solids Probe Quadrupole Mass Spectroscopy, MS of Gases, Calvet and Heat Loss Calorimetry

See Table 1 for a list of calculated binding energies of hydrino hydride ions for the first 14 stages (*n* parameter) of hydrino collapse. The text mentions *n* values up to 200.

Hydrides may lead to high value chemicals in the fields found in Table 2.

BLP Power Technology

BlackLight Power cells can be grouped into four categories (see Table 3). The Website has drawings of test setups for each of these cells.

Commentary

Among the initiatives in the new energy field, Mills and BLP have made a lot of the right moves to attract financial backing and to proceed toward a potentially commanding market position with long-term pay back to the investors. He may eventually be ranked among Eastman, Land, Carlson, and Edison as a technical entrepreneur who built an industry on a personal invention. These built on existing scientific knowledge, but Mills has done something more in his Grand Unified Theory. Such theories are ambitious and apt to be dismissed by critics, but its predictive power should draw the most careful attention, even though it may be modified in the future.

At present BLP has shown evidence of hydrinos, hydrino hydrides, intense energy release, and UV radiation in small scale tests. All this is significant. Commercial success of BLP requires scale-up to large and replicated reactors.

Originally BLP's strategy was based on power generation, retrofitting the boilers in existing utilities and designing smaller units for transportation. At present in the U.S. and many of the developed nations, fuel is cheap and there is little incentive to undertake the capital costs of a transition to a whole new power system.

Discovery of the hydrino hydrides changed all that, particularly the potential of the battery technology. If the theoretical promise can be realized, then the philosophy of power generation and distribution could change. For example, electric automobiles using BLP batteries might outperform internal combustion engines and run 1000 miles on a charge.

These systems will require extensive engineering. Much

Table 1.

Hydride Ion	Binding Energy, eV	Hydride Ion	Binding Energy, eV
H^- (n = 1/2)	3.047	H^- (n = 1/9)	42.83
H^- (n = 1/3)	6.610	H^- (n = 1/10)	49.37
H^- (n = 1/4)	11.23	H^- (n = 1/11)	55.49
H^- (n = 1/5)	16.70	H^- (n = 1/12)	60.97
H^- (n = 1/6)	22.81	H^- (n = 1/13)	65.62
H^- (n = 1/7)	29.34	H^- (n = 1/14)	69.21
H^- (n = 1/8)	36.08	H^- (n = 1/15)	71.53

Table 2. Applications of Hydrino Hydride Compounds
Batteries (Automotive and Consumer), Etching Agents, Optical Coatings, Polymers and Synthetic Fibers, Masking Agents, Optical Filters, Isotope Separation, Semiconductor Fab., Agents to Dopants, Fiber Optic Cables, Refining Methods, Negative Ion of Electrolyte of a High Voltage Electrolytic Cell, Superconductors, Xerographic Compounds, Photoluminescent Compounds, Explosives, Propellants and Solid Fuels, Magnets and Magnetic Computer Storage Media, Photovoltaics, Cathodes for Thermionic Generators, Corrosion Resistant Coatings, Photoconductors, Industrial Cutting Materials, Heat Resistant Coatings, Phosphors for Lighting, Light Weight, High Strength Structural Materials, Proton Source, Thermionic Generator, Light Source

New Energy Times Archive

Table 3.

Power Cell Type	Catalyst	Dissociator	Operating Temperature Range	Typical Power Density	Application Areas
Electrolytic	K_2	Ni cathode and Ni or Pt anode	25 - 100 C	1 mW/cm2	Space Heating Long term production of hydrino hydrides
Low Temperature Gas	KNO_3	precious metal	250 - 350 C	50 mW/cm^2	comparable to a nuclear power plant, lower maintenance, no radioactive waste
High Temperature Gas	KI	transition metal, precious metal, or refractory metal	700 - 1200 C	100 mW/cm^2	fossil fuel boiler, hydrino hydride reactor
Ultra-high Temperature Gas	KI	refractory metal	1200 - 2000 C	in progress	hydrino hydride production, self sustaining operation

is in place, but the production and distribution of the necessary equipment will require some years of work.

The power yield of a given BLP reactor is strongly dependent on the autocatalysis sequence, where hydrinos catalyze other hydrinos to still lower energy levels. This is quite new, yet it must be managed to sustain the expected power levels. For example, the gas phase reactor operates at low pressure to minimize recombination of hydrogen atoms to molecules. Low pressure also reduces the probability of catalytic encounters between H and K^+ atoms. It also reduces the energy density and limits transfer of heat to the outside to radiation. Optimizing these factors could be the basis of future patents or trade secrets to maintain a controlling position after the basic patents expire.

In the chemical case, similar process control problems exist. The reactors can be expected to produce hydrino populations with a spread of degrees of collapse. These will have different binding energies and different chemical properties. Separation and purification of such a mix is a classical chemical engineering problem, with a whole new chemistry to deal with. The toxicity, if any, of these new compounds may come into question if widely used.

The potential rewards of the BLP technologies will justify the combined efforts of BLP and members of alliances it may form with various industries. BLP's announced intention to hire 80 to 100 Ph.D.'s in the next two years shows an awareness of this problem. They will be needed.

Mills has maintained a careful distance from the "cold fusion" field, maintaining that the BLP process is exclusively chemical, having nothing to do with nuclear phenomena. In the current Website text, "cold fusion" is dismissed as a failed effort.

Readers of *IE* are well aware of the problems associated with replication of the Fleischmann and Pons effect, the variability of cathode materials, and the difficulty of replication and scale-up. Yet, through the work of Arata and Zhang, Case and others, it is clear that a second set of energy processes exist which do not seem to involve the BLP process – catalysis and orbital collapse. In the A&Z cells, deuterium ions participate in the reaction. In the Case cells, no catalyst with the specified energy hole is present (unless specific catalysis conditions exist at the fractured surfaces of the palladium film). The energy densities in these reactions can be very high, with evidence of micro-explosions and melting in cathode materials.

Mills has staked out a claim to this territory in his Australian patent by noting that deuterium can also form hydrinos. Being smaller, and electrically neutral, the deuterons can approach closely with increase in the fusion probability. He terms this process Coloumbic Annihilation Fusion. It should be noted that if catalytic orbital collapse can occur for hydrogen, it should occur for other elements, leading to uncharted areas of chemistry.

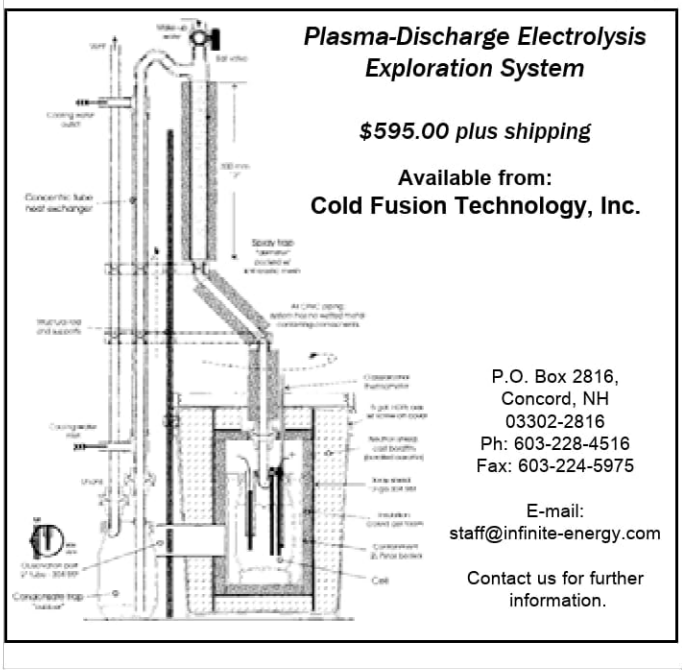
The extensive family of transmutation reactions which are collectively known as Low Energy Nuclear Reactions, or Chemically Assisted Nuclear Reactions, appear to lie outside the BLP universe.

The emergence of novel products and devices which demonstrate the new knowledge are keys to kindling a paradigm shift in physics to usher in the twenty-first century.

Editorial continued from page 14

investment in the new energy field.

Ending where this began, we return to George Gamow's musings of 1966, when I was a sophomore at MIT in aero/astro engineering. Gamow thought that the next major physics revolution would be in understanding the very existence of elementary particles. He wrote, "There is hardly any doubt that when such a breakthrough is achieved, it will involve concepts that will be as different from those of today as today's concepts are different from those of classical physics." He was both wrong and right. He could not have suspected that the next physics revolution would begin not with high energy particle physics but with fundamental electrochemistry—and that it would end with the birth of modern alchemy. The revolution will be the end of the world that we have known, this time for the better.



New Energy Times Archive

Cold Fusion Comix

— A variety of humorous insights about the serious matter of New Energy, New Physics,

"Look—there goes one of those U. F. O.'s again."

Reprinted with permission from Sidney Harris

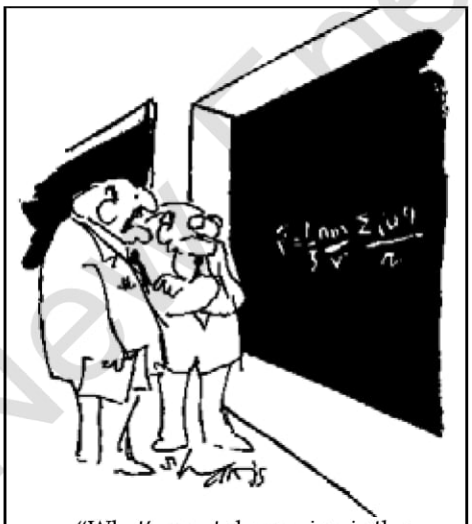
BAR STOOL SCIENCE NUMBER 4 • **BY SUSAN SEDDON**

Computer Journal of Thomas A. Edison

It is with deep sadness that today I must destroy the time machine. My most astonishing invention and no one will ever know about it. Ironic how they all dismissed the device as a useless vote recording machine when, with a few minor tweaks here and there, I was able to vote yea or nay on whether to visit the twentieth century. A great pity too that I can't spread around the latest Senate gossip from that era. Not that they'd believe a word of it if I did! I suppose it's my own fault; I was careless. That irritating Nikola Tesla stumbled across the Windows 95 CD ROM. I told him it was a failed attempt to find a new way to record sound but I don't think he bought it. I'll fix him one of these days! Anyway, who would believe I never used the time machine to steal 1990's technology and apply it to my inventions? They'd all think I was a crook and who could blame them? They'd certainly never believe I could only travel to one future date, New Year's Eve 1999 and was always zapped back at midnight, like Cinderella, due to that darned millennium bug. Who would believe the truth: that I got hooked on disco and only kept going there for the light show and that wonderful music so loud even I couldn't fail to hear every note. I'll really miss those funky rhythms. The only things I brought back were this portable computer someone left lying around and a satellite phone which is quite useless back here. It's not as though I could leave the Function Room, not even to visit the bathroom. I've still never touched a drop since the trip when I made that discovery—that was one occasion when midnight couldn't come fast enough for me. I never managed to have a serious discussion with any of the other revelers either. I defy anyone to hold a productive technical debate with a bunch of advertising executives. It was good to see my inventions still in use in the twentieth century though. So, my time machine and these few trappings of 1999 must be destroyed without a trace. I've already thrown the cursed Windows 95 CD-ROM onto the fire, something I should have done years ago. I first installed it way back in 1868 and I still haven't managed to iron out all the bugs.

Next Issue: The Dead Scientists Poetry Society

Reprinted with permission from Sidney Harris



"What's most depressing is the realization that everything we believe will be disproved in a few years."

"Practice Random Acts of Extreme Physics" —Ed Wall

Submitted by: Oscar Kelley

"Alien Pyramid Scheme" Scientists Decode the First Message From an Alien Civilization. . .

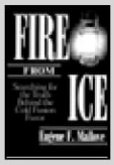
Simply send 6 x 10^{-50} atoms of hydrogen to the star system at the top of the list, cross off that star system, then put your star system at the bottom of the list and send it to 100 other star systems. Within one-tenth of a

Your ad could be here. Only $90 for a 1/6 page ad size: 4 5/8" X 2 5/16"
Call:
Cold Fusion Technology, Inc.
(603)228-4516

FIRE FROM ICE:
Searching for the Truth Behind the Cold Fusion Furor

By Eugene F. Mallove, Sc.D.
Published by John Wiley & Sons, Inc., 1991, 334 pp.
$25.95 No. America; $29.95 Other Foreign
(Includes Shipping/Airmail)

Fire From Ice appeared on the bookshelves of the MIT Student Centers Tech. Coop in 1991.



Cold Fusion Technology, P.O. Box 2816,
Concord, NH 03302-2816;
Phone: 603-228-4516; Fax: 603-224-5975;
e-mail: staff@infinite-energy.com

New Energy Times Archive

COMPILED BY EUGENE MALLOVE

SSE Annual Meeting to Feature Cold Fusion

The SSE (Society for Scientific Exploration) will hold its 18th annual meeting June 3-5, 1999 at the University of New Mexico at Albuquerque. Some of the possible speakers at the meeting are: Edmund Storms, George Miley, Thomas Claytor, Melvin Miles, Michael McKubre, and Peter Graneau. The main theme sections of the meeting are:

- Shaking the Pillars of the Paradigm
- Cold Fusion
- Reports on Anomalous Phenomena
- Suppression of New Science

Registration details may be found at: http://www.jse.com/eighteenth.html

RCCNT-7 Seventh Russian Conference on Cold Nuclear Transmutation

Sponsored by:

Russian Academy of Science, Russian Physical Society, Nuclear Society of Russia, Mendeleev Russian Chemical Society, Lomonosov Moscow State University, Russian People's Friendship University, Moscow State Technical University (MADI)

First Announcement

Dear Colleague,

We are now organizing the Seventh Russian Conference on Cold Nuclear Transmutation (RCCNT-7), which will take place in hotel Olimpiyskiy in Dagomys near Sochi (one of the best Russian retreats on the shore of the Black Sea) during the week of May 23-30, 1999.

The Conference program will include the following subjects: 1. Experimental research on cold fusion and nuclear transmutation; 2. Cold fusion and nuclear transmutation theoretical models; 3. Cold fusion applied technologies and devices.

I am pleased to invite you to participate in this conference and report on any subject that you wish. Please send the abstract of the report you wish to present. The languages of the Conference will be Russian and English. If you wish to take part in our Conference, you should inform us by e-mail or fax (before March 1, if possible). I will be able to make your visa and hotel arrangements. Please provide your e-mail address.

The registration fee of $900 for participants covers: conference proceedings and program; transportation from Sochi Airport to the hotel and back; a hotel stay in Sochi; daily meals (breakfast, dinner and supper) during the May 23-30 period; and the organizing fee. If you pay before March 1, the fee may be reduced to $800. We will be able to meet you at the Sochi Airport on May 23 if you provide information about your flight number. The projected date of return flights from Sochi Airport is May 30.

Sincerely,
Yury N. Bazhutov
Co-chairman of the RCCNT-7
Organizing Committee

Phone: (095) 464-78-81, (095) 946-15-51
P.O. Box 169, "Erzion" Center, 105077
Moscow, Russia Fax: (095) 151-89-65
e-mail: prfnart@hotmail.com

Science Frontiers Festival Held in France

by Jean-Pierre Lentin
jplentin@club-internet.fr

The Science Frontiers Festival (16th edition) took place January 27-31, 1999 in Cavaillon (South of France). Definitely a good year! There was record breaking attendance (800 seats in a big hall, always full) and unprecedented media coverage—with all TV networks coming to visit.

The Science Frontiers Festival: It was open to a large audience (hence the designation "Festival") and it was also a meeting place for conventional and alternative scientists, with lots of tolerance and good "vibes" on both side. Presenting controversial or alternative ideas in science is only one of the Festival's goals. The others are assessing the impact of recent scientific advances, and discussing present planetary problems—with a strong accent on environment/ecology.

Here is a brief report of events at the festival, concerning cold fusion and new energy:

Over-unity energy and cold fusion were featured for the first time at the festival and were an *instant hit!*

Jean-Louis Naudin demonstrated the SMOT and a few small motors, gave a SMOT-building course and generally impressed everybody with his precise and unassuming . . . approach. [Editor's Note: See Mike Carrell's report on Naudin's work in *Infinite Energy* No. 23.—EFM]

Physicist Patrick Cornille presented his theoretical views on *stimulated forces*, which might account for over-unity. Cornille has a regular scientific job with state-funded CEA (Atomic Energy Commission), but his theoretical and experimental work on non-conventional electromagnetics is a personal pursuit. As he is nearing retirement age, he can be expected to work full-time on over-unity in the years to come, and his deep knowledge of physics (as witnessed by his contributions to Szames' book—see below) will surely be a big asset.

Naudin and Cornille seemed highly confident of the future. They recently founded a company, Advanced Electromagnetic Systems, which will soon have its own web page, and Cornille has a huge theoretical paper to be published in a mathematical physics journal. Stay tuned. [Editor's Note: See a paper by Cornille in *Infinite Energy* No. 21.—EFM]

Jean-Paul Biberian was a big success too. His brand of Woody Allenesque humor had the audience rolling in laughter. His contract for cold fusion experiments with the Atomic Energy Commission will end in a few months. He has had many positive, if small-scale, results; he has original ideas for new techniques, so he is definitely "hooked" and wants to keep on researching CF. Next year he might return to the U.S., if work opportunities permit. Don't miss him if he's lecturing somewhere!

Biberian was also seen in discussions into the wee hours of the night with Science Frontiers' favorite maverick physicist, Joel Sternheimer, who received a "Scientist of the Year" prize at the end of the Festival. Sternheimer has a patent on a quite revolutionary technology—sound frequencies and melodies that stimulate or inhibit the synthesis of specific proteins, with medical and agricultural

Briefs continue on page 48

New Energy Times Archive

MY LIFE WITH COLD FUSION AS A RELUCTANT MISTRESS

Talk given at "Cold Fusion and New Energy Symposium, October 11, 1998, Manchester, New Hampshire

Edmund Storms
2140 Paseo Ponderosa, Santa Fe, NM 87501 USA

Over nine years have passed since many of us were lured into believing that the Pons-Fleischmann effect would solve the world's energy problems and make us all rich. Things have not yet worked out as we had hoped. Each of us have followed a different path through the labyrinth of this expectation. I would like to share with you my particular path and show you how I came to believe that problems of reproducibility are caused solely by the properties of the materials in which the nuclear reactions are proposed to occur.

When the announcement was made, I was working at the Los Alamos National Laboratory (LANL) on rather conventional materials problems associated with trying to design nuclear reactors for use in space. Suddenly, the possibility of making nuclear energy in a Mason jar was the center of attention and conversation at LANL. Meetings were held and memos flowed freely—typical of a government operation. Excitement was intense, causing previously untapped creative juices to flow and an intensity of communication within the laboratory not seen since the War. Dozens of imaginative experiments were started using funds otherwise destined for the design of better atomic bombs. In my case, a search for tritium production seemed to be the most logical approach, because I was located in a building where worked some of the world's experts in the properties and detection of tritium. They know tritium when they see it. So, with the financial backing of the DOE, my future wife (Carol) and I set about trying to verify the claims for tritium production using the Pons-Fleischmann effect.

Our approach was to electrolyze heavy-water in a closed cell designed to collect tritium produced in the electrolyte in a separate compartment from that present in the evolving gas. Because we planned to do many experiments, in order to explore a wide range of variables, the cells were designed to be cheap and simple. As you can see in Figure 1, the evolving D_2 and O_2 pass through a recombiner and the resulting D_2O is collected in an IV bag. For a brief time, we were the major user of IV bags in Los Alamos. The method allowed us to keep a complete inventory of all material within the cell including any tritium. Over 250 cells were studied using palladium from many different sources containing many different introduced impurities. Unfortunately, only thirteen cells produced excess tritium and the amount was rather small. Typical results obtained from active and inactive cells run at the same time are shown in Figure 2. Note the delay in tritium production followed by a rapid onset with bursts. In the process, we made an important discovery. When tritium is produced, it always appears first in the electrolyte, not in the evolving gas. This is important because it indicates that tritium is being produced only at the surface and it leaves the sample before it has a chance to dissolve in the metal lattice. The implications of this behavior will be described in more detail a little later. Unfortunately, we could find no relationship between its production and the nature of the palladium, hence the effect was not reproducible. Being highly controversial, the resulting paper[1] was reviewed by twelve people at LANL and by several more reviewers after it was submitted to *Fusion Technology*. Even though this intense scrutiny found no fatal flaws, the results, although published after peer review, were universally ignored. The papers that skeptics publish should be so carefully analyzed.

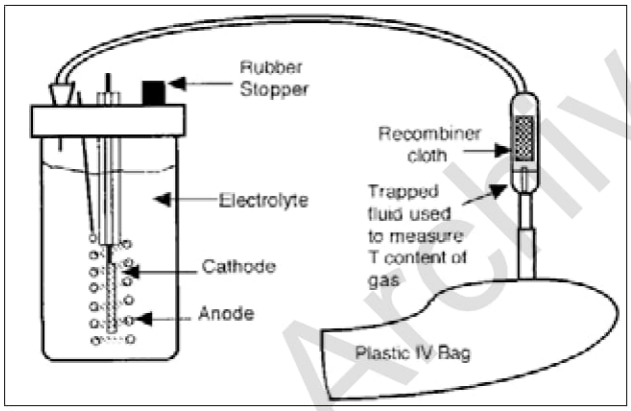


Figure 1. Drawing of closed cell used to study tritium production.

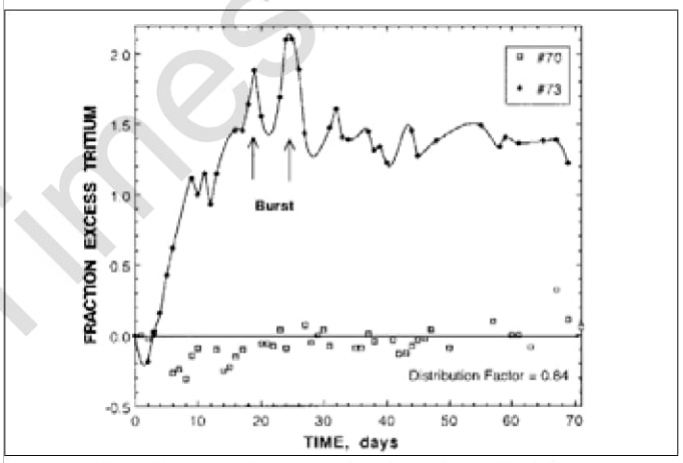


Figure 2. Typical behavior of active and inactive cells in which tritium was measured. Both cells were run at the same time. Note the four day delay before tritium was produced in cell #73.

One of the major rationalizations used to reject such work was the assumption that tritium was already in the palladium or came from the surrounding environment. Consequently, we set about to test these assumptions. We had already made numerous attempts to find dissolved tritium within the initial palladium and within the environment without success. So we chose the opposite approach. We placed cells in an environment known to contain tritium. In addition, we dissolved a known amount of tritium in palladium destined for electrolytic study. If the behaviors seen during the proposed cold fusion production matched those found using known tritium additions, then an explanation would be obvious.

When a cell was placed in a tritium environment, the tritium content of the electrolyte slowly increased at a linear rate, as expected (Figure 3). In contrast, anomalous tritium was seen to occur only after a delay and then was produced at a high rate over a short time with bursts (Figure 2). In addition, similar cells in the same, clean environment showed no effect while tritium was growing in a neighboring cell. Consequently, the observed behavior of cold fusion production of tritium and that obtained from the

New Energy Times Archive

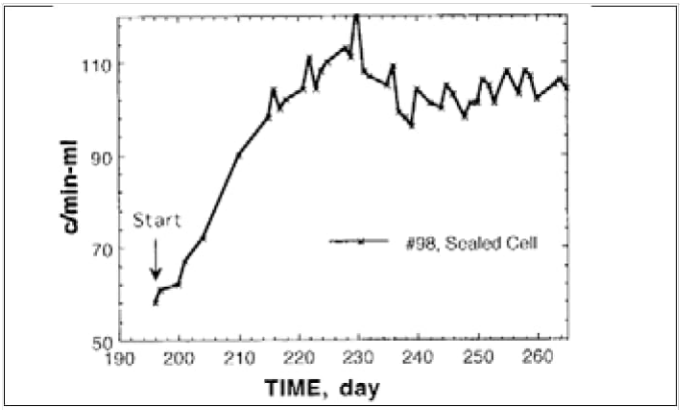


Figure 3. Pickup of tritium in a sealed cell attached to an IV bag located within an environment containing tritiated water vapor.

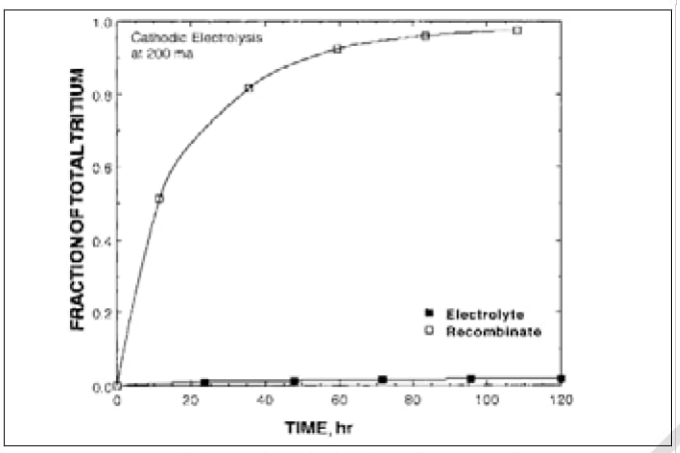


Figure 4. Growth of tritium in the electrolyte and in the evolving gas as a sample of palladium containing tritium was electrolyzed as the cathode.

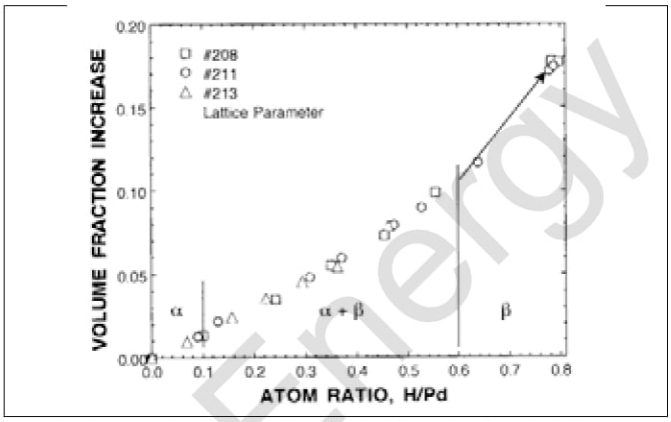


Figure 5. Increase in volume produced by loading palladium with hydrogen.

environment show entirely different patterns of behavior.

Tritium which was placed in the palladium on purpose always appeared first in the evolving gas, not in the electrolyte, during electrolysis. As can be seen in Figure 4, the contained tritium was released immediately after electrolysis started and continued at a steadily reduced rate as electrolysis continued. Loss into the gas was caused by a first order reaction for which a half-life could be determined. Again, the behavior of tritium claimed to be produced by cold fusion and the behavior of known tritium were different. To us at least, this study demonstrated that neither contaminated palladium nor the environment were the source.[2] Skeptics were forced to propose that tritium released in "cold fusion" cells was not dissolved but was present as isolated impurities to which tritium was tightly bonded. Never explained was the mechanism of its subsequent rapid release after hours of electrolysis. Unfortunately, this work was also completely ignored.

Individual studies always have errors which rightly introduce doubt. On the other hand, a series of studies using different approaches that produce patterns of behavior are much more difficult to reject. In this case, the observed patterns are completely consistent with anomalous tritium being produced within the cell and, more specifically, in the surface of the palladium cathode. Nevertheless, people of a skeptical mindset would propose terribly unlikely and convoluted processes to explain individual results while ignoring the patterns. Amazingly, this method of rejection seems to satisfy many people in academic science these days.

During this study, we began to appreciate how important crack formation was in determining the local concentration of deuterium. Most palladium grows an increased crack concentration each time it is loaded. This crack concentration is measured as a volume increase over that expected from the lattice parameter change, shown in Figure 5. Only a very few samples did not show this effect. Using tritium as a tracer, we were able to quantify the effect of cracks on the loss rate of hydrogen from palladium. In general, the higher the excess volume, the faster tritium left the sample through the cracks as gas, thereby entering the gas rather than being dissolved in the electrolyte as ions. In this case, tritium was also being used as a tracer for deuterium loss. Thus, both tritium and deuterium, previously dissolved in the PdD, left the structure through cracks as gas rather than by ion exchange at the surface. Only tritium produced by the anomalous reaction entered the electrolyte. Consequently, we could conclude that most palladium acted like a leaky bucket which could never be filled. Since we know that high deuterium loading is a requirement for excess energy production, these cracks are apparently a major hindrance in achieving the required high deuterium concentration. Unfortunately, this work had no influence on the skeptics and very little influence on those people who were also trying to reproduce the effect.

About a year later, I wrote a review[3] which was published in *Fusion Technology*. Using this collected experience as an argument to look for heat production, I convinced my Division Leader at LANL to fund a calorimetric study. The calorimeter was sealed, closed and stirred, all requirements demanded by various skeptics before claims could be believed. A drawing of the device is shown in Figure 6. For our first study, I was given a piece of palladium from a batch made by Tanaka Metals which had been shown to produce excess energy by Prof. Takahashi in Japan. Amazingly, his sample also produced excess energy in my calorimeter. The most dramatic of the various examples of excess heat production from this sample is shown as a function of time in Figure 7. As you can imagine, this unexpected result was analyzed every which way by numerous people to discover the source of the apparent energy. The calorimeter was calibrated using an internal heater many times during the study and three times during excess energy production, with no apparent change. Unfortunately, the study had to be terminated prematurely because the internal recombiner began to fail. After the calorimeter was repaired, some additional excess energy was seen, but the sample soon died. Because we were sensitive to the influence of cracks, we measured this piece and found very few of the little devils (1.7%). A second piece of palladium from a different batch, but one claimed to have been made in the same manner, was delivered from Japan and studied. This piece was found to produce no excess energy, but contained a high concentration of cracks (13.5%). After informing the Japanese of this fact, a third batch was made. But this time,

New Energy Times Archive

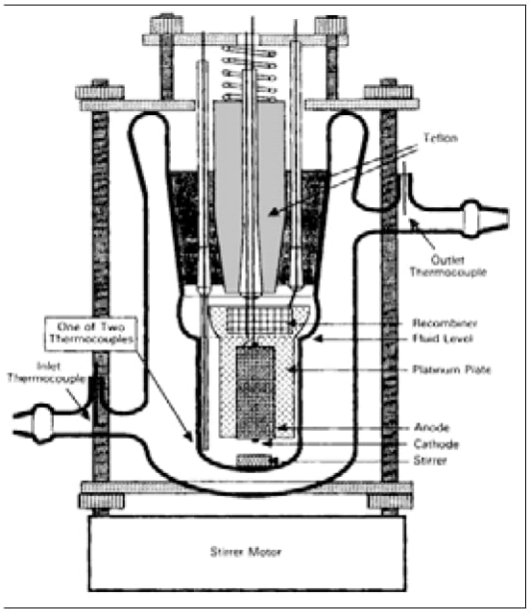


Figure 6. Drawing of the calorimeter used at LANL. The cell was sealed, contained a recombiner, and was stirred. Temperature was measured at two positions within the cell. The deuterium content of the palladium cathode was determined by measuring the change in deuterium pressure.

the conditions were as close as those used to make the first batch as was possible. This material was found to make excess energy, although less than the first batch. Also, it was found to contain a crack concentration slightly greater than the first batch. From these results, it is clear that the crack concentration is an important variable. Table 1 compares the behavior of several samples made in Japan. While crack concentration (excess volume) is important, it is not the only variable having influence on excess energy production.

Table 1
Comparison between measured properties and excess power production.

Sample Number	Excess Volume, %	Composition D/Pd	OVC*	Excess Power, W (at 3 A)
Tanaka 1	1.7	0.82		7.5
IMRA #38	2.8	0.875	1.03	3.2
Tanaka 3	2	0.84		2
IMRA #42	1 to 2	0.891	1.25	4.6
IMRA #84	6.7	0.752	1.00	1.5
IMRA #58	4.1	0.833	0.60	0.0
Tanaka 2	13.5	0.75		0.0

* Open circuit voltage

Once again, this information, when published,[4] had very little effect on other people's work. It did, however, start me down a path to answer the question, "Why is the effect so difficult to produce?" In addition, this experience started a search for ways to pretest palladium. Meanwhile, conventional science was taking the opposite approach—saying because the effect could not be reproduced at will, it was not real.

About this time several events occurred to give many of us some optimism. Thanks to Eugene Mallove, Rep. Dick Swett (NH) asked me to testify before a congressional subcommittee which was reviewing future funding for "hot fusion."[5] Unfortunately, this effort had no effect on future actions by the government toward "cold fusion." Then, in spite of increasingly vocal opposition to the whole idea, *MIT Technology Review* took the courageous step of asking me to write an article for them.[6]

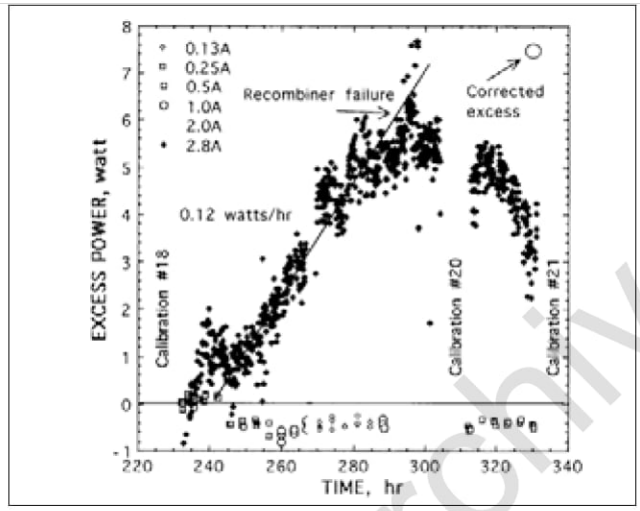


Figure 7. Time variation of excess power production using Takahashi palladium with various applied currents. The calorimeter was calibrated before, during, and after the study. No significant changes were noticed. Excess power was observed only above a critical current. The "correct excess" value is calculated assuming no recombination is occurring at that time.

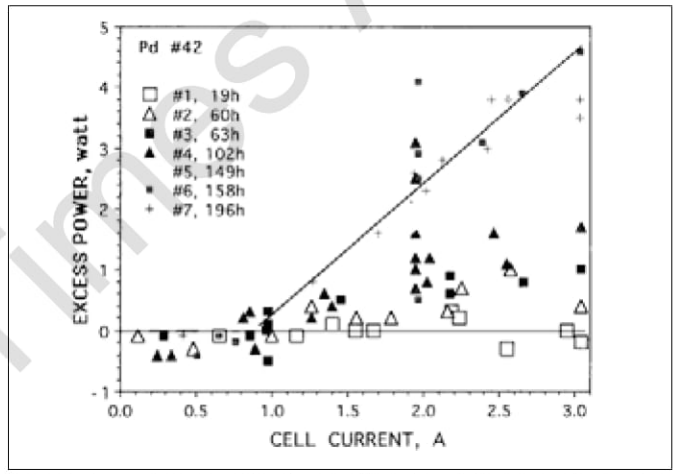


Figure 8. Excess power as a function of applied current after electrolyzing for various times.

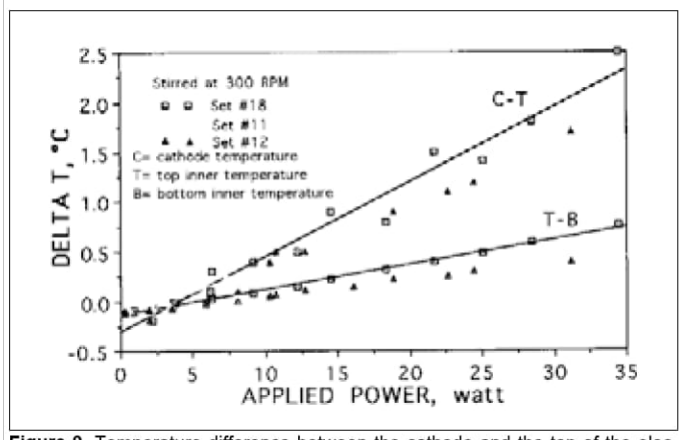


Figure 9. Temperature difference between the cathode and the top of the electrolyte and between the top and the bottom of the cell. No excess power was detected during Sets #11 and #12. Excess power was detected during Set #18.

Although this article stirred up some heated debates, it also had no significant impact on the course of the field.

My work in cold fusion at LANL was meeting increased resistance and my new wife and I wanted to build a home in Santa Fe. Retirement began to look very attractive. Amazingly, they were even willing to pay me a bonus to leave. After we

New Energy Times Archive

finished the house and a laboratory, I began to study ninety pieces of palladium furnished by IMRA (Japan). Some of this material produced excess energy as shown in Figure 8 and listed in Table 1. In addition, a temperature gradient was found to occur within the cell during excess energy production such that the cathode appeared to be the source of energy, as shown in Figure 9. This sample was found to be unique in that the low excess volume did not grow larger upon repeated deloading-loading cycles. Energy production was very difficult to kill, returning after a short delay even when the surface was removed by Aqua Regia. The study also demonstrated that palladium could be pretested, thereby reducing the growing frustration by eliminating most material from which energy could never be obtained. This study was published in *Infinite Energy*[7] [Ed. note: *Errata* regarding figures in this previously published paper to appear in a forthcoming issue]. In addition, the study showed cracking to be a highly variable property and pointed out a number of other variables, besides cracking, as being important. These included the deloading rate after the current was stopped, the open-circuit-voltage, and the loading efficiency. As my laboratory grew, thanks to Dave Nagel (NRL) and Fred Jaeger (ENECO), computer control could be introduced allowing these variables to be explored in more detail. Unfortunately, the results of this study[8] were held up for about a year by the then editor of *Journal of Electroanalytical Chemistry* (JEAC), apparently because the paper mentioned cold fusion. The work was recently published in *J. Alloys and Compounds* and described at ICCF-7.

These studies provided insights into the nature of the regions in which the nuclear reactions actually occur. These regions I call a special condition of matter containing nuclear-active-states (NAS).[9] It is interesting to examine some of these results in detail. In order to produce "cold fusion" or more exactly "chemically assisted nuclear reactions" (CANR), the hydrogen isotope must achieve a very high local concentration. To do this, the atoms must get into the metal lattice though a surface barrier, and the atoms must stay in the lattice regardless of there being many avenues for escape. The "getting in" and the "getting out" are independent variables having wide ranges of values which depend strongly on the nature of the metal. Once regions of high-concentration are achieved, changes must take place such that the atomic and electronic structures are altered to produce unique conditions—a new chemical phase. This transition is also sensitive to a variety of conditions in addition to the hydrogen concentration. In short, two major conditions must be achieved—a high hydrogen concentration and then conversion to a new phase having a composition well above that of normal β-PdD. Each of these conditions is affected by dozens of variables. It is little wonder that the effect is difficult to reproduce. The "going in" can be examined using the loading efficiency; this quantity being the ratio created when the number of atoms of hydrogen presented to the surface by the applied current is divided into the number which actually dissolve in the metal, each measured over a five minute interval. Figure 10 shows the behavior of an untreated sample and one whose surface barrier was reduced using Aqua Regia. Needless to say, a poor "going in" rate needs to be avoided. The "going in" rate is also sensitive to applied current and temperature. The "going out" rate can be studied by measuring the deloading rate after the current is turned off. This rate is obtained from a plot of average composition vs. square root of time, as shown in Figure 11. The initial slope is used to evaluate samples. The slopes for numerous samples are compared in Figure 12 as a function of maximum average composi-

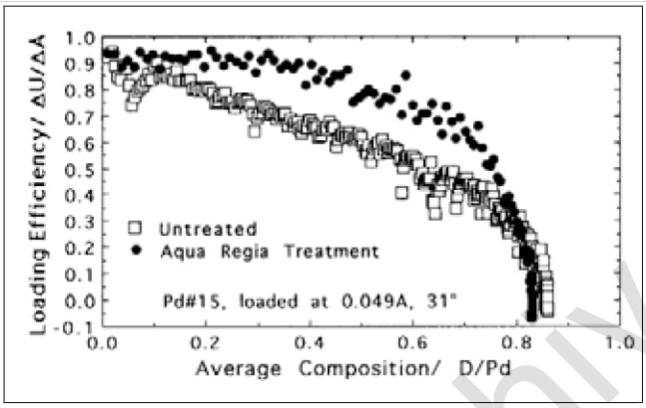


Figure 10. Effect of a surface barrier on the loading efficiency.

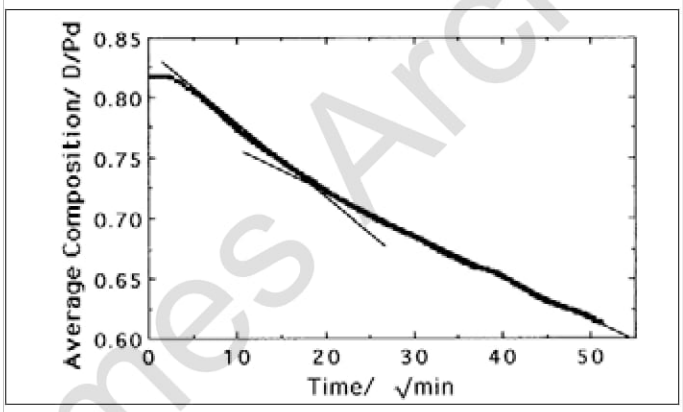


Figure 11. Change in average composition vs square root of time after applied current is stopped. The initial delay is caused by inertia in the composition measuring system. A break in slope occurs when α-PdD forms on the surface.

tion. Two conclusions are obvious: the loss rate increases as the initial composition is increased and some samples have an abnormally low rate even though they have a high composition. From this and other arguments, I conclude that this same loss is occurring while current is being applied. In addition, the rare piece of palladium which is able to achieve very high average compositions does so because it has a low loss rate, in addition to several other features. Consequently, both the loading efficiency and the deloading rate can be combined to quickly identify potentially nuclear-active palladium, in addition to using material having low excess volume.

Unfortunately, the average composition used to obtain these two quantities is not the important variable. The highest composition exists on the surface and it is this composition that determines whether a sample will become nuclear-active. Fortunately, changes in surface composition can be estimated using the open-circuit-voltage (OCV), a value which is sensitive to the chemical activity of hydrogen in the surface. A value is obtained by measuring the voltage between the cathode and a platinum reference electrode while the current is turned off for a few seconds. Figure 13 shows how the OCV changes as a palladium sample is loaded with deuterium. A reverse of this behavior is frequently seen when a sample is allowed to deload after the current is turned off. However, occasionally, especially after very high average compositions have been achieved, the OCV shows a different behavior as seen in Figure 14. This behavior indicates that a new phase has formed which slowly decomposes into normal β-PdD as deuterium is lost. The next question needing an answer is: "What is the actual composition of this phase?" A partial answer can be obtained by studying very thin films of palladium plated on to platinum. As shown in Figure 15, the measured composition of such films is highly

New Energy Times Archive

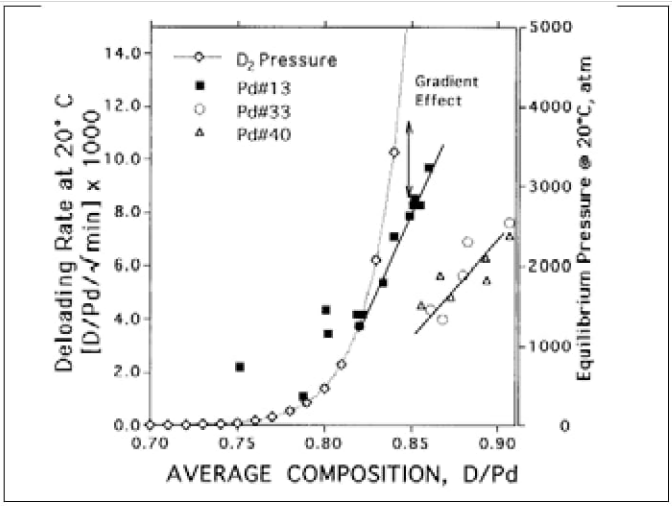


Figure 12. Variation of deloading rate as a function of average composition. Also shown is the equilibrium pressure within voids for the indicated compositions. The gradient effect is caused by a reduced composition at the surface of a crack produced by deuterium loss.

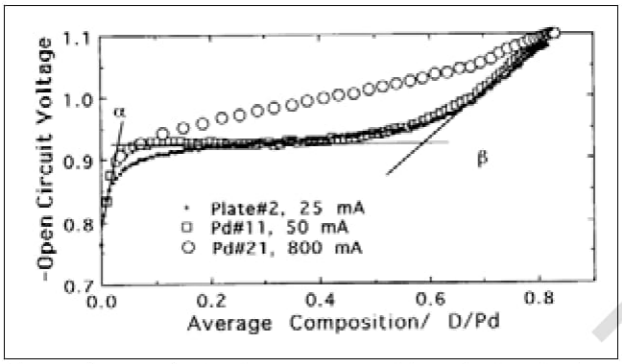


Figure 13. Open circuit voltage measurement during loading at various currents referenced to platinum.

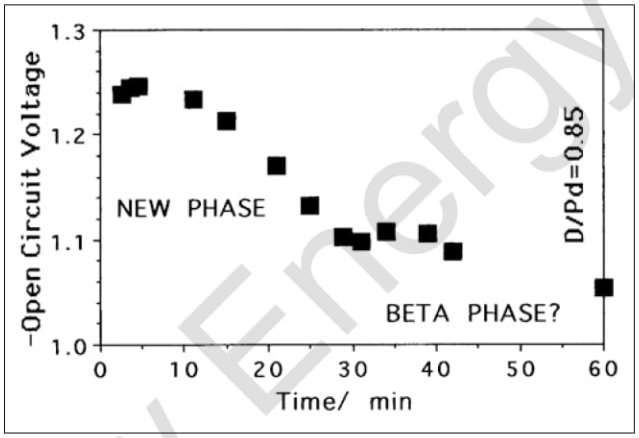


Figure 14. Open circuit voltage during deloading after production of excess energy.

variable but can achieve a composition as high as D/Pd=1.5. In this case, the surface region containing the high composition is not as diluted by the smaller interior composition as would be the case if a thicker sample were used, such as shown by the lower curve. Since this particular film was not nuclear-active, the composition of a nuclear-active surface is probably significantly higher. In addition, deloading from a surface is very nonuniform, as can be seen by examining bubble production. Therefore, the maximum composition of such a thin film, or indeed any palladium surface, is well above the average value in addition to being above D/Pd=1.5. This observation means that all theories based on the properties of β-PdD are barking

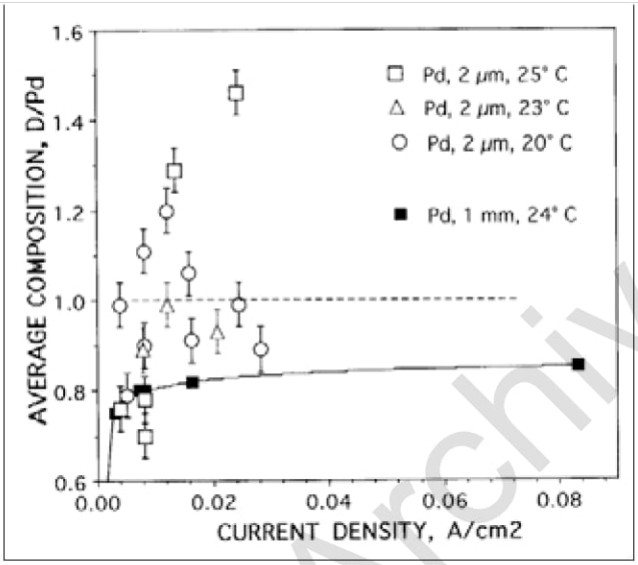


Figure 15. Measured average composition of thin films of Pd plated on Pt after being subjected to different current densities. The behavior of a typical thick plate is also shown.

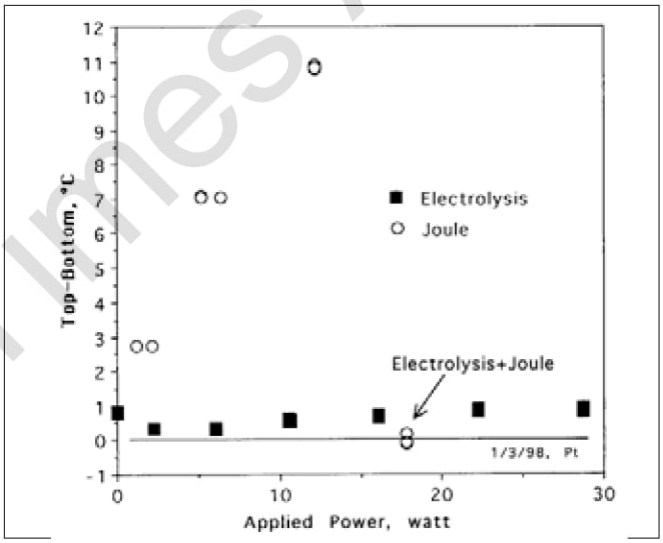


Figure 16. Gradient between the top and bottom of the electrolyte as a function of applied power.

up the wrong tree. I have proposed the actual nuclear-active phase to be PdD_{2+x}.[9]

Meanwhile, the results of this study were used by ENECO in an attempt to convince the Patent Office that the lack of reproducibility was caused by limitations inherent in the chosen material. This argument was completely ignored. In addition, I wrote another review in an attempt to bring all of the better data under one roof so that even an open-mind-challenged person could see the bigger picture. This was published in *J. Scientific Exploration*[10] with brief versions in *21st Century Science and Technology*.[11] and in an earlier volume of *J. Scientific Exploration*.[12] Again, no change was produced at the Patent Office nor in any other government agency. Recently, a third and final review was rejected by the *International J. of Modern Phys.* even though one of the Editors requested the paper. Amazingly, four reviewers rejected its publication for reasons that appeared to me to be based on a bias against cold fusion. This work was published in *Infinite Energy*.[13] You can decide for yourselves whether this review deserved such treatment. My recent experience suggests that many avenues for publication previously available no longer provide the service because reviewers have

New Energy Times Archive

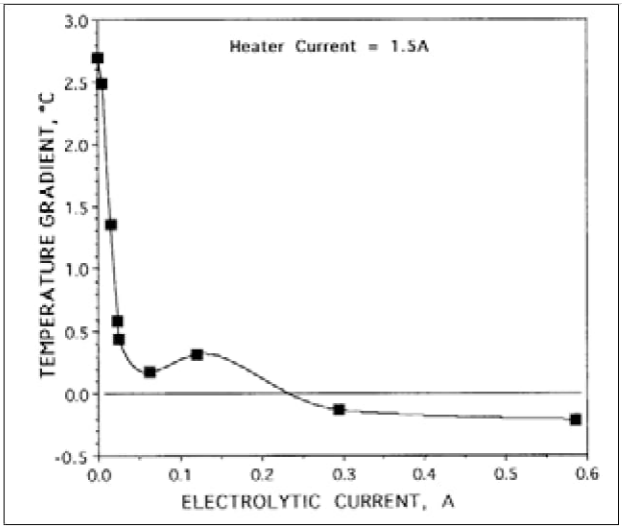


Figure 17. Reduction in gradient as electrolysis current is increased at a fixed heater current.

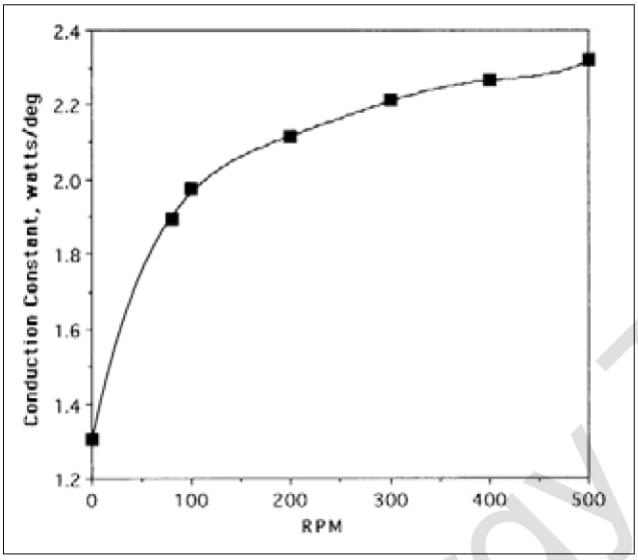


Figure 18. Thermal conduction of the cell wall as a function of stirring speed using an isoparibolic calorimeter.

become even more unwilling to give cold fusion the benefit of doubt. It is depressing to see resistance increase as the experimental results and understanding improve.

Last year, Dr. Naoto Asami sent me some palladium that the New Hydrogen Energy Laboratory (NHE) had made at great expense and to their specifications. Unfortunately, they could detect no excess energy using this material. As expected, my tests showed this material to be flawed—material I would expect to be completely inert. After hearing this, Dr. Asami invited me to Japan where we were able to discuss the problem in some detail. Apparently, impurities were being introduced into the palladium during manufacture as well as during subsequent annealing. We were able to eliminate the impurities added by annealing. Unfortunately, insufficient time and money were available to change the method of manufacture. Consequently, NHE closed down without exploring the use of proven, active palladium.

Recently, I have been exploring the various errors besetting calorimetric measurements. I have discovered that stirring is not an important variable when electrolysis is used, in contrast to what has been claimed by some well know skeptics. This can be seen in Figure 16 where the gradients produced by an internal heater and by an electrolysis current are shown. A small

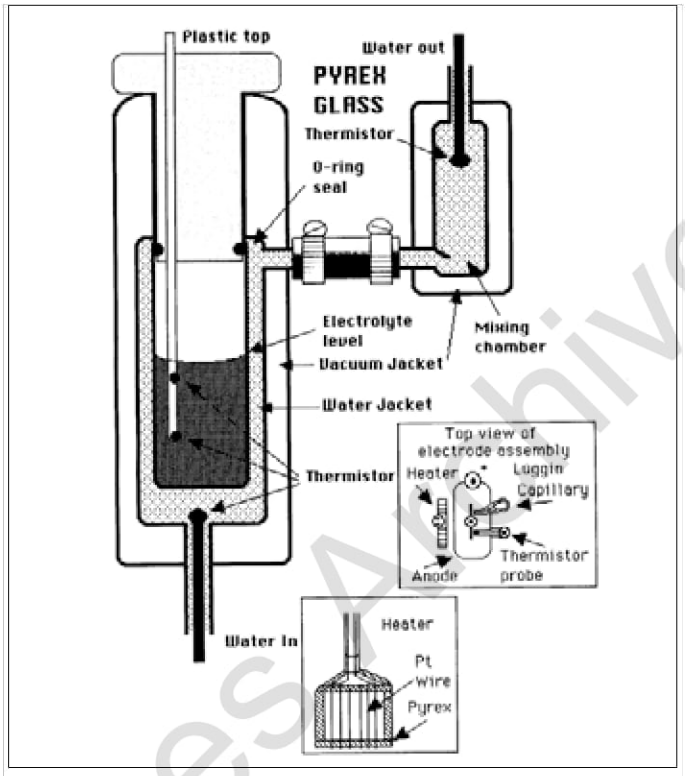


Figure 19. Drawing of the dual calorimeter. The cell is sealed, contains a recombiner, and a Joule heater. Temperature is measured at three locations within the cell.

electrolysis current added to the Joule heating reduces the gradient to insignificant values (Figure 17). On the other hand, the stagnate layer of fluid at the cell wall is important when isoperibolic methods are used. This layer influences the thermal conductivity of the wall and is very sensitive to the amount of fluid convection. Figure 18 shows how the calibration constant changes as the stirring rate is increased. In the absence of mechanical stirring, bubble generation serves the same end although not to the same degree. This study shows that claims for small amounts of heat using unstirred isoperibolic calorimeters may be in error. Pons and Fleischmann largely avoided this problem by frequently calibrating their cell, although the effect may still have some influence on their data especially at the lowest claimed excess power levels. Other studies, especially if they use Joule heating to calibrate the cells, are not so fortunate. In my work, calibration was based on electrolytic heating, with Joule heating being used only to determine whether the calibration constant had changed while electrolysis was ongoing.

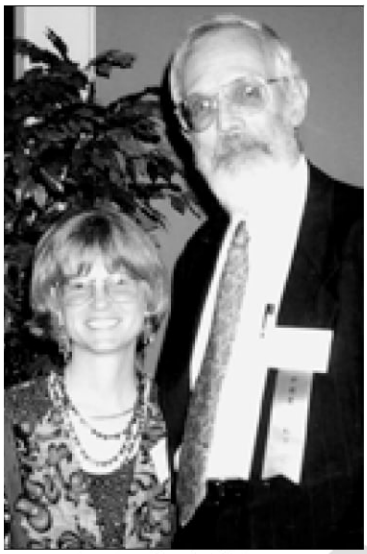
I have also built a dual calorimeter (Figure 19) which is being used to study electroplated palladium on platinum and search for evidence of superconductivity in nuclear-active-material. This method greatly reduces the likelihood of misinterpreting apparent excess energy. Electroplated palladium appears to have a much greater likelihood of success compared to bulk material, and it is cheaper.

What can I conclude from this experience? First, the phenomenon claimed by Pons and Fleischmann is real, but it is only a small part of a much larger picture. The reality of this phenomena has an even greater importance to science and technology than was ordinarily proposed. Second, the method used by Pons-Fleischmann is useless for eventual production of commercial power. Active palladium is too difficult to find and conditions are too sensitive to impurities. Nevertheless, it is a very useful and inexpensive method to explore certain aspects of the phenomena. It is unfortunate that these brave and creative thinkers had to take so much pain and be denied the rewards of

New Energy Times Archive

their discovery by closed-minded colleagues and an incompetent Patent Office. And third, the field is sick and on life support. Major sources of financial support have dried up, many self-funded individuals are moving on, and conventional rejection has solidified. Those of us who would like to see this field grow are encouraged by a few impressive successes, such as Case, Arata, and Stringham, but even these approaches are woefully underfunded and are being studied by a very small number of individuals. We most hope that when the spectacular demonstration demanded by skeptics is found, a way is available to make this fact generally known.

The amazing claims for transmutation are getting increased attention and are accumulating experimental support, but acceptance is hard to find even in the cold fusion community. These claims require expensive tools to show their reality—money that is not generally available. The only solutions are for the Patent Office to change its approach and/or for individuals or companies to provide funds toward a basic understanding without an immediate guarantee of financial return—several very unlikely possibilities. The most likely possibility is that the entire field, heat and transmutation, will get noticed only after the price of oil skyrockets in the next few years or if there is a major spill of highly radioactive material from a waste dump. Meanwhile, a very objective article in *Wired* magazine[14] will at least wake up the public to this issue. Hopefully, some awareness will trickle down from the public to the scientific profession, the reverse of the usual procedure.


Drs. Carol and Ed Storms

As for me, I will continue to study the effect as time and money permit. However, additional useful knowledge demands sophisticated techniques that I presently do not have available. This limitation makes continued study of this effect a matter of diminishing returns for me. Therefore, I am gradually turning my attention to other problems—such as earning a living and preparing for Y2K. Giving up this mistress is hard for me but very attractive to my wife, as you might expect. Eventually, the field will be discovered by other people, hopefully by someone who can afford to marry her.

References

1. E. K. Storms and C. Talcott, "Electrolytic Tritium Production," *Fusion Tech.*, 17 (1990) 680.
2. E. K. Storms and C. Talcott-Storms, "The Effect of Hydriding on the Physical Structure of Palladium and on the Release of Contained Tritium," *Fusion Tech.*, 20 (1991) 246.
3. E. K. Storms, "Review of Experimental Observations about the Cold Fusion Effect," *Fusion Tech.*, 20 (1991) 433.
4. E. K. Storms, "Measurements of Excess Heat from a Pons-Fleischmann Type Electrolytic Cell Using Palladium Sheet," *Fusion Tech.*, 23 (1993) 230.
5. Hearing before the Subcommittee on Energy of the Committee on Science, Space, and Technology U.S. House of Representatives, May 5, 1993, #38, p. 114.
6. E. K. Storms, "Warming Up to Cold Fusion," *MIT Technology Review*, May/June 1994, page 19.
7. E. Storms, "A Study of Those Properties of Palladium That Influence Excess Energy Production by the Pons-Fleischmann Effect," *Infinite Energy*, 2, #8 (1996) 50.
8. E. K. Storms, "Formation of β-PdD Containing High Deuterium Concentration Using Electrolysis of Heavy-Water," *J. Alloys and Compounds* 268 (1998) 89.
9. E. K. Storms, "The Nature of the Energy-Active State in Pd-D," *Infinite Energy*, 1, #5 & 6, (1996) 77.
10. E. K. Storms, "A Review of the Cold Fusion Effect," *J. Sci. Exploration*, 10, #2 (1996) 185.
11. E. K. Storms, "Cold Fusion, An Outcast of Science," *21st Century Science & Technology*, Winter 1997/1998, page 19.
12. E. K. Storms, "Cold Fusion: A Challenge to Modern Science," *J. Sci. Exploration*, 9, #4 (1995) 585.
13. E. K. Storms, "Cold Fusion Revisited," *Infinite Energy*, 4, #21 (1998) 16.
14. Charles Platt, "Dirty Science: The Strange Rebirth of Cold Fusion," *Wired*, Nov. 1998, page 171.

✧✧✧

Briefs continued from page 41

applications. As a mathematical physicist, Sternheimer is developing a new twist in quantum theory, where the particle waves are not simply sinusoidal, as in classical QM, but have harmonics too. So he thinks that in CF, hydrogen electrons might jump to a lower harmonic, thus achieving fractional quantum states (*a la* Mills or Vigier), getting closer to the nucleus and gaining energy.

Alexandre Szames presented his big fat book (500 pages, three years work) on the Biefeld-Brown effect. This twenty-nine year-old writer has an impressive scientific vitae (puts me to shame, easily!) and did fascinating experimental work with help from Naudin and Cornille. They conclude that Biefeld-Brown effect is NOT anti-gravity, but a crucial proof of the existence of aether (thus invalidating general relativity) and stimulated forces, opening the way to over-unity and new propulsion concepts. Just after the Festival, Szames flew to the U.S. to participate in NASA's latest seminar on breakthrough propulsion in Albuquerque, New Mexico. I expect some news from him when he returns.

Lastly, another sign that O/U or FE research is gaining ground in France: we finally have a French language web site on these topics, thanks to Jean and Bernadette Soares and their association, Quant, Homme. For years they did a wonderful job translating articles and information, and now they have a site full of links and references, at [http://multimania.com/quanthomme].

Full program is available (in French only) at :
http://www.transfert.net/mag/reportag/pages/m1frepor.html

CONFERENCE ON FREE ENERGY

U.S. Department of State, 2201 C Street, NW,

Washington, DC 20520

Acheson Auditorium, April 29 and 30, 1999 9 a.m. - 6p.m.

Co-sponsored by Integrity Research Institute

For further information and schedule of events:

call IRI at 800-295-7674, fax: 301-513-5728,

e-mail iri@erols.com

Pre-registration required for admission.

See page 121 for more information.

New Energy Times Archive

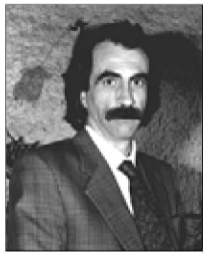
Theoretical Indications of the Possibility of Nuclear Reactions at Low Energy

E. Conte

Centro Studi Radioattività, Via Dante Alighieri 256, 70122 Bari - Italy; E-mail conte@teseo.it

Abstract

It is evident that the cold fusion of a proton and an electron into a neutron, here considered as a natural bound state of particles at low temperatures, is expected on the basis of a generalization of Schrödinger's equation. Also the barrier penetrability for d-d reactions has been evaluated on the basis of such a generalized equation.



Introduction

In 1920[1] E. Rutherford introduced an historical hypothesis about the structure of the neutron. The proton was represented as a sphere having a radius equal to that of its charge distribution (1F) filled up with the wavepackets of the constituents. The neutron was thought of as a kind of "compressed hydrogen atom," with the electron immersed in the dense medium of the proton. The aim of this paper is to show that Rutherford's model may be re-interpreted in the framework of quantum mechanics using biquaternions.

A Brief Note On Biquaternions

The description of physical phenomena requires the use of algebraic structures. There are emerging indications that Clifford algebra offers a framework for a unified setting of relativistic fields. The biquaternions pertain to the Clifford algebra. They have been introduced in physics by us[2] \as an attempt to delineate and to generalize quantum mechanics.

Let us sketch the basic framework for the algebraic construction of biquaternions. This Clifford algebra may be defined by some anticommuting bases. Consider the set (e_0, e_1, e_2, e_3), where e_0 is a scalar unit and e_1, e_2, e_3 are three anticommuting basic elements. Admitting a scalar square for each element $(e_0e_0 = e_0)$; $e_0e_0 = 1$; $e_1e_1 = k_1$; $e_2e_2 = k_2$; $e_3e_3 = k_3$; with $k_1 \neq 0$; $k_2 \neq 0$; $k_3 \neq 0$

and $e_ie_j = - e_je_i$ with $i = 1, 2, 3$ and $j = 1, 2, 3$; $i \neq j$, (1.1)

we may write the multiplication of such elements in the following manner

$$e_1e_2 = a_1e_1 + a_2e_2 + a_3e_3 ;$$
$$e_2e_3 = \beta_1e_1 + \beta_2e_2 + \beta_3e_3 ;$$
$$e_3e_1 = \gamma_1e_1 + \gamma_2e_2 + \gamma_3e_3 . \quad (1.2)$$

In addition to anticommutativity of e_1, e_2, e_3, we impose left and right alternativity in the following manner

$$e_1\, e_1\, e_2 = (e_1\, e_1)\, e_2;\;\; e_1\, e_2\, e_2 = e_1\, (e_2\, e_2)$$
$$e_2\, e_2\, e_3 = (e_2\, e_2)\, e_3;\;\; e_2\, e_3\, e_3 = e_2\, (e_3\, e_3)$$
$$e_3\, e_3\, e_1 = (e_3\, e_3)\, e_1;\;\; e_3\, e_1\, e_1 = e_3\, (e_1\, e_1) . \quad (1.3)$$

Using (1.2) and (1.3), we have that

$$k_1e_2 = a_1k_1 + a_2\, e_1\, e_2 + a_3e_1e_3 ;$$
$$k_2e_1 = a_1\, e_1\, e_2 + a_2\, k_2 + a_3e_3e_2 ; \quad (1.4)$$

or, equivalently,

$$k_1\alpha_1 + e_1(\alpha_2a_1 - \gamma\alpha_3) + e_2(\alpha_2^2 - k_1 - \gamma_2\alpha_3) + e_3\,(\alpha_2\alpha_3 - \gamma_3\alpha_3) = 0$$
$$k_2\alpha_2 + e_1(\alpha_1^2 - k_2 - \beta_1\alpha_3) + e_2(\alpha_1\alpha_2 - \beta_2\alpha_3) + e_3(\alpha_3\alpha_1 - \beta_3\alpha_3) = 0 \quad (1.5)$$

From Equation (1.5) we obtain that

$$\alpha_1 = \alpha_2 = \beta_2 = \beta_3 = \gamma_1 = \gamma_3 = 0; \quad \alpha_3 \neq 0, \quad \beta_1 \neq 0, \quad \gamma_2 \neq 0 . \quad (1.6)$$

In conclusion, we may calculate that

$$k_1 = - \gamma_2\alpha_3; \quad k_2 = - \beta\alpha_3; \quad k_3 = - \beta_1\gamma_2$$

and $e_1e_2 = a_3e_3$; $e_2e_3 = \beta_1e_1$; $e_3e_1 = \gamma_2e_2$. (1.7)

The possible different values for k_i ($k_i = \pm 1$) originate Clifford algebra. We are interested in the case $k_1 = k_2 = k_3 = + 1$ that gives $\alpha_3 = \beta_1 = \gamma_2 = i$, and

$$e_1e_1 = e_2e_2 = e_3e_3 = + 1$$
$$e_1e_2 = - e_2e_1 = ie_3 ; \quad e_2e_3 = - e_3e_2 = ie_1 ; \quad e_3e_1 = - e_1e_3 = ie_2$$
$$e_0e_1 = e_ie_0 = e_i \quad (i= 1, 2, 3) \quad (1.8)$$

The set (e_0, e_1, e_2, e_3) of Equation (1.8) constitutes the four generators for the algebra here considered.

Let us give now a proper definition of biquaternion. It is a quaternion having complex components

$$Z = z_0e_0 + z_1e_1 + z_2e_2 + z_3e_3 \quad (1.9)$$

We may chose $e_0 \equiv 1$; the basic elements e_i $(i= 1, 2, 3)$ satisfy the Pauli-algebraic relations $e_i^2 = 1$; $e_i \neq 1$; $e_ie_j = -e_je_i = i\, e_k$;

$$i= 1, 2, 3 ;\; j= 1, 2, 3 ;\; k = 1, 2, 3 ; \quad i \neq j \quad (1.10)$$

with (i, j, k) cyclic permutation of $(1, 2, 3)$.

z_μ $(\mu = 0, 1, 2, 3)$ are complex numbers $(x_\mu + i\gamma_\mu)$. A real biquaternion is defined to have the imaginary parts of z_μ equal to zero. The hyperconjugate Z^* of Z is defined as it follows

$$Z^* = (x_0 + iy_0) - \sum_{i=1}^{3} (x_i + i\, y_i)\, e_i \quad . \quad (1.11)$$

The complex conjugate Z^+ and the quaternion conjugate of $\overline{Z}$ of Z are defined as it follows

$$Z^+ = (x_0 - iy_0) + \sum_{i=1}^{3} (x_i - i\, y_i)\, e_i \quad (1.12)$$

and

$$\overline{Z} = (x_0 - iy_0) - \sum_{i=1}^{3} (x_i - i\, y_i)\, e_i \quad . \quad (1.13)$$

Each basic element of the set (e_0, e_1, e_2, e_3) has its inverse: e_1, e_2, e_3, respectively.

The biquaternions have a norm defined in the following manner

$$N(Z) \equiv Norm(Z) = Z^*Z = Z\,Z^* = z_0^2 - z_1^2 - z_2^2 - z_3^2 \quad . \quad (1.14)$$

We have that $Norm(A\, B) = N(A)\quad N(B)$. The inverse of Z is $Z^{-1} = Z^*/ N(Z)$ with $N(Z) \neq 0$.

An interesting property of biquaternions is that they admit zero divisors. When the norm of Z is zero, $N(Z) = 0$, but $Z \neq 0$, we call Z a zero divisor.

The biquaternions may be transformed according to Linear Biquaternion Transformations (LBT). Let Z and Z' be biquaternions before and after transformation. The most general LBT is given to be

$$Z' = A\, Z\, B + C \quad (1.15)$$

where A, B, C are any biquaternions satisfying in case the normalization conditions $N(A) = N(B) = N(C) = 1$. Linear Homogeneous Biquaternion Transformations (LHBT) are given from the (1.15) for $C = 0$. It is of particular interest the LHBT given by $C = 0$, $A = U$, $B=U^+$; $N(U\, U^+) = N(U^+U) = 1$. It is immediately necessary to show that LBT and LHBT form a group.

Let us examine now the most important property of the biquater-

New Energy Times Archive

nions. It is immediately necessary to verify the existing isomorphism between the algebra of Pauli matrices and that one of the basic elements for such hypercomplex numbers. According to (1.10), we may identify $e_0 \equiv 1$ and the basic elements e_i (i = 1, 2, 3) with a matrix representation; we have that

$$e_0 = \begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix}; \quad e_1 = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix}; \quad e_2 = \begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix}; \quad e_3 = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix} . \tag{1.16}$$

In this case all the algebra of the biquaternions holds, and we have a matrix representation of such numbers at order $n = 2$.

Consider now the following matrix representation of e_0 and of the basic elements e_i (i = 1, 2, 3) :

$$E_0 = \begin{pmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 0 \end{pmatrix}; \quad E_1 = \begin{pmatrix} 0 & 1 & 0 \\ 1 & 0 & 0 \\ 0 & 0 & 0 \end{pmatrix}; \quad E_2 = \begin{pmatrix} 0 & -i & 0 \\ i & 0 & 0 \\ 0 & 0 & 0 \end{pmatrix}; \quad E_3 = \begin{pmatrix} 1 & 0 & 0 \\ 0 & -1 & 0 \\ 0 & 0 & 0 \end{pmatrix}$$

$$E_4 = \begin{pmatrix} 0 & 0 & 1 \\ 0 & 0 & 0 \\ 1 & 0 & 0 \end{pmatrix}; \quad E_5 = \begin{pmatrix} 0 & 0 & -i \\ 0 & 0 & 0 \\ i & 0 & 0 \end{pmatrix}; \quad E_6 = \begin{pmatrix} 1 & 0 & 0 \\ 0 & 0 & 0 \\ 0 & 0 & -1 \end{pmatrix}; \quad E_7 = \begin{pmatrix} 0 & 0 & 0 \\ 0 & 0 & 1 \\ 0 & 1 & 0 \end{pmatrix}$$

$$E_8 = \begin{pmatrix} 0 & 0 & 0 \\ 0 & 0 & -i \\ 0 & i & 0 \end{pmatrix}; \quad E_9 = \begin{pmatrix} 0 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & -1 \end{pmatrix}; \quad \overline{E}_8 = \begin{pmatrix} 1 & 0 & 0 \\ 0 & 0 & 0 \\ 0 & 0 & 1 \end{pmatrix}; \quad \overline{E}_9 = \begin{pmatrix} 0 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{pmatrix} . \tag{1.17}$$

In this case we have that the algebra of the biquaternions holds for the following sets: (E_0, E_1, E_2, E_3), $(\overline{E}, E_4, E_5, E_6)$ and $(\overline{E}_0, E_7, E_8, E_9)$, and we have a matrix representation of such complex numbers at the order $n = 3$. Let us observe also that (1.17) are the well known Gell-Mann matrices. Also of interest is the case of matrix representation of biquaternions for $n = 4$. We have the following elements:

$$E_{00} = e_0 \otimes e_0 = \begin{pmatrix} 1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 1 \end{pmatrix}; \quad E_{01} = e_0 \otimes e_1 = \begin{pmatrix} 0 & 1 & 0 & 0 \\ 1 & 0 & 0 & 0 \\ 0 & 0 & 0 & 1 \\ 0 & 0 & 1 & 0 \end{pmatrix}; \quad E_{02} = e_0 \otimes e_2 = \begin{pmatrix} 0 & -i & 0 & 0 \\ i & 0 & 0 & 0 \\ 0 & 0 & 0 & -i \\ 0 & 0 & i & 0 \end{pmatrix};$$

$$E_{03} = e_0 \otimes e_3 = \begin{pmatrix} 1 & 0 & 0 & 0 \\ 0 & -1 & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & -1 \end{pmatrix}; \quad E_{10} = e_1 \otimes e_0 = \begin{pmatrix} 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 1 \\ 1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \end{pmatrix}; \quad E_{20} = e_2 \otimes e_0 = \begin{pmatrix} 0 & 0 & -i & 0 \\ 0 & 0 & 0 & -i \\ i & 0 & 0 & 0 \\ 0 & i & 0 & 0 \end{pmatrix};$$

$$E_{30} = e_3 \otimes e_0 = \begin{pmatrix} 1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & -1 & 0 \\ 0 & 0 & 0 & -1 \end{pmatrix}; \quad E_{13} = e_1 \otimes e_3 = \begin{pmatrix} 0 & 0 & 0 & 1 \\ 0 & 0 & 1 & 0 \\ 0 & 1 & 0 & 0 \\ 1 & 0 & 0 & 0 \end{pmatrix}; \quad E_{31} = e_3 \otimes e_1 = \begin{pmatrix} 0 & 0 & 0 & -1 \\ 0 & 0 & 1 & 0 \\ 0 & 1 & 0 & 0 \\ -1 & 0 & 0 & 0 \end{pmatrix};$$

$$E_{33} = e_3 \otimes e_3 = \begin{pmatrix} 1 & 0 & 0 & 0 \\ 0 & -1 & 0 & 0 \\ 0 & 0 & -1 & 0 \\ 0 & 0 & 0 & 1 \end{pmatrix}; \quad E_{12} = e_1 \otimes e_2 = \begin{pmatrix} 0 & 0 & 0 & -i \\ 0 & 0 & i & 0 \\ 0 & -i & 0 & 0 \\ i & 0 & 0 & 0 \end{pmatrix}; \quad E_{11} = e_1 \otimes e_1 = \begin{pmatrix} 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & -1 \\ 1 & 0 & 0 & 0 \\ 0 & -1 & 0 & 0 \end{pmatrix};$$

$$E_{21} = e_2 \otimes e_1 = \begin{pmatrix} 0 & 0 & 0 & -i \\ 0 & 0 & -i & 0 \\ 0 & i & 0 & 0 \\ i & 0 & 0 & 0 \end{pmatrix}; \quad E_{31} = e_3 \otimes e_1 = \begin{pmatrix} 0 & 1 & 0 & 0 \\ 1 & 0 & 0 & 0 \\ 0 & 0 & 0 & -1 \\ 0 & 0 & -1 & 0 \end{pmatrix}; \quad E_{23} = e_2 \otimes e_3 = \begin{pmatrix} 0 & 0 & -i & 0 \\ 0 & 0 & 0 & i \\ i & 0 & 0 & 0 \\ 0 & -i & 0 & 0 \end{pmatrix};$$

$$E_{32} = e_3 \otimes e_2 = \begin{pmatrix} 0 & -i & 0 & 0 \\ i & 0 & 0 & 0 \\ 0 & 0 & 0 & i \\ 0 & 0 & -i & 0 \end{pmatrix} . \tag{1.18}$$

Note that $E_{11}, E_{12}, E_{13}, E_{30}$ are just the matrices in Dirac's equation for the electron. We have now five possible sets that may be used for matrix representation of biquaternions. They are given as it follows (E_{00}, E_{01}, E_{02}, E_{03}); (E_{00}, E_{10}, E_{20}, E_{30}); (E_{00}, E_{11}, E_{22}, E_{33}); (E_{00}, E_{12}, E_{31}, E_{23}); (E_{00}, E_{21}, E_{13}, E_{32}).

Note that the basic elements are given according to the cyclic permutation (i, j, k) of $(0, 1, 2, 3)$ with -0, $i \to i$; i, $0 \to i$. In addition, we have that $e_i \otimes e_j = E_{i0}E_{oj} = E_{ij}$. Thus, in the case $n = 4$, we have an important property for the basic elements of the biquaternions: given the two basic sets (E_{00}, E_{0i}) and (E_{00}, E_{io}) $(i = 1, 2, 3)$ the other sets are generated by their multiplication.

In conclusion, we have obtained that a matrix representation of the biquaternions is possible at various orders $n = 2, 3, 4$. For the purposes of the present paper we may limit our discussion remaining at the order $n = 4$ of the biquaternions. However, the following statement holds: we may find a matrix representation of the biquaternions at any order n, $n = 2, 3, 4...$

THE BASIC PHYSICAL FEATURES OF THE BIQUATERNION QUANTUM MECHANICS:

An Analysis of the Generalized Schrödinger's Equation

We may use Linear Homogeneous Biquaternion Transformations (LHBT) as a useful method to investigate some aspects of the physical theory, and, in particular, as a useful tool to have a generalization of quantum mechanics. We have called Biquaternion Quantum Mechanics (BQM) such a generalization of the standard quantum theory.[2] The BQM has some important features; one aspect regards the possibility of the BQM to introduce a generalization of Schrödinger's equation. It is this generalized Schrödinger's equation that we will use in the present paper. Let us summarize the results.[2]

Let us start by a generalization of the standard commutator of quantum mechanics. Let us consider the LHBT given with $C=0$, $A=U=S^{1/2}(-i-e_1)$ $B=U^+=S^{1/2}(i-e_1)$ where $S \equiv S(q,t)$ is some scalar function. We have that

$$U[p_k,q_j]U^+ = Up_kU^+QUq_jU^+ - Uq_jU^+QUp_kU^+ = Up_kU^+q_j - q_jUp_kU^+ = -i\hbar\delta_{jk}S . \tag{2.1}$$

Here it is $U\,U^+=2S$; $N(U)=-2S$; $I=U\,U^+$; $Q=I^{-1}$. The generalized commutator is

$$[p_k,q_j] = -\,i\hbar\delta_{jk}\,S \text{ with } p_k = Up_kU^+ = -\,i\,\hbar\,(\delta S/\delta q_k) \tag{2.2}$$

In order to obtain a generalization of Schrödinger's equation, we consider the canonical Hamilton-Jacobi equations for one particle, $\delta A/\delta t + H = 0$; $\delta A/\delta x_k = p_{ak}$ where $A = -\,i\hbar \log\psi$ is the canonical action.[2] Let us generalize A by the same LHBT, obtaining $A' = UAU^+ = -\,i\hbar\,S\log\psi$, and

$$\bar{H} = i\hbar\,(dS/d\,t)\log\bar{\psi} + i\hbar\,(S/\psi)\,(d\psi/d\,t);\ -i\hbar\,\nabla\psi = [p + i\hbar\,(\nabla S)\log\psi]\,S^{-1}\psi \tag{2.3}$$

A generalized equation may be now written in the following manner

$$[H - i\hbar\,(\delta S/\delta\,t)\log\psi]S^{-1}\psi = i\hbar\,(\delta\psi/\delta\,t) \ \text{ or } \ H_1S^{-1}\psi = i\hbar\,(\delta\psi/\delta\,t) = H_1\psi';\ \ \psi' = S^{-1}\psi \tag{2.4}$$

where we have obtained that

$$H \to H_1 = H - i\hbar\,(\delta S/\delta\,t)\log\psi;$$

$$p_k \to \overline{p_k} = p_k + i\hbar\,(\nabla S)\log\psi;\ \ \psi \to \psi' = S^{-1}\psi \tag{2.5}$$

Equation (2.4) represents the generalized Schrödinger's equation of the BQM as it was previously obtained.[2] It seems that a large class of different physical models is unified in one and only one structure of the generalized Schrödinger's equation as identified in (2.4) by using biquaternions in BQM. In fact, some authors[3, 4, 5, 6] identified equations in agreement with Equations (2.2) and (2.4) and thus with the elaboration of the present paper.

Let us go to solve (2.4), assuming $\delta S/\delta t = 0$. Consider the solution $u \equiv u(r)$ of the conventional Schrödinger equation

$$H_u(r) = [H_0 + V(r)]u(r) = E_u u(r) \tag{2.6}$$

connected to (2.4). We utilize this solution to introduce the non-local operator $S^{-1} = 1 - |u\rangle\langle u|$. Introducing it in (2.4), we obtain

$$[H_0 + V(r) + V_H(r)]\psi = E\psi \tag{2.7}$$

where the "pseudo potential" $V_H(r)$ is given to be

$$V_H(r) = -\,E_u < u\,/\,\psi > u\,/\,\psi\ . \tag{2.8}$$

Let us observe the important result that we have obtained. The generalized Schrödinger's Equation (2.4) involves the use of a pseudopotential $V_H(r)$ given in (2.7). This pseudopotential depends explicitly on the integral of wave overlapping

New Energy Times Archive

$$a_s = \langle u / \psi \rangle \qquad (2.9)$$

We have a new profile for the physical processes with $a_s \neq 0$. In addition to the conventional, local, action-at-distance, potential interactions indicated by $V(r)$ in (2.7), we have to add, as existing in Nature, other interactions that are non-linear, non-local, and non-potential interactions, and they are due to the wave-overlapping as explicitly indicated in Equation (2.9). The BQM considers physical processes that are marked by an actual overlap of the wave functions. In these cases, the generalization (2.7) of Schrödinger's equation is required, and it may be rewritten in the following comprehensive biquaternion form

$$H\begin{pmatrix}1 & 0\\ 0 & -S^{-1}\end{pmatrix}\begin{pmatrix}u\\ \psi\end{pmatrix} = \begin{pmatrix}E_u & 0\\ 0 & -E\end{pmatrix}\begin{pmatrix}u\\ \psi\end{pmatrix} \qquad (2.10)$$

where we have used the biquaternions $H/2\ (1 - S^{-1}) + H/2(1 + S^{-1})\ e_3$, and $(Eu - E)/2 + [\ (Eu + E)/2]\ e_3$. The central mechanism expected for Equations (2.8) and (2.10) is the deep mutual penetration $\langle u|\psi \rangle \neq 0$ of the wave packets. Let us start calculating Equation (2.7) when we consider $V(r) = -e^2/r$. In this case the wave function u(r) has the form $u(r) \cong C_1\ exp(-br)$. We may obtain an important result by numerical methods. Using Hartree-like self-consistent field approximation, we solve (2.7) iteratively, and we find

$$\psi(r) \cong F(r)\ [1 - exp(-br)] \qquad (2.11)$$

with initial point r_0 such that $F(r_0) \cong b$, and $F(r) \cong C_2\ exp(-\alpha r)/r$. Considering Reference 7, page 176, we immediately verify that it must be

$$V_H(r) = -V_{0exp}(-br)/[1 - exp(-br)] = - Eu\alpha_s u/\psi\ . \qquad (2.12)$$

The conclusion is that for $V(r) = - e^2 / r$, $V_H(r)$ is an Hulthén potential, well known from nuclear physics.

ON A POSSIBLE GENERALIZATION OF THE RELATIVISTIC INVARIANT OF SPECIAL RELATIVITY

Let us consider now another important feature of biquaternions: *they have an intrinsic relativistic character. The norm of a biquaternion fully reproduces the basic invariant that is known in special relativity.* As shown in detail in Reference 2, we may consider a *biquaternion space* consisting of all the biquaternions with a quadratic metric defined by the norm of the biquaternion. To a given biquaternion

$$Z = z_0e_0 + z_1e_1 + z_2e_2 + z_3e_3 \qquad (3.1)$$

corresponds the point $P(z_\mu)$ $(\mu = 0, 1, 2, 3)$ in the complex four-dimensional space (z_0, z_1, z_2, z_3) and vice-versa. The subset consisting only of real biquaternions is the real biquaternion space where the norm coincides with the quadratic metric of Minkowski space.

Let us remain in the field of the real biquaternions. Consider the biquaternion

$$Z = x_0 + x_1e_1 + x_2e_2 + x_3e_3 \qquad (3.2)$$

and an LHBT(*Z*) given as it follows

$$Z' = UZU^+ = x'_0 + x'_1\, e_1 + x'_2\, e_2 + x'_3\, e_3 \qquad (3.3)$$

where $U = a + be_1 + ce_2 + de_3$

and $U^+ = a^* + b^*e_1 + c^*e_2 + d^*e_3.$ (3.4)

According to Reference 2, if we consider the case $b = c = 0$, $a \neq 0$, $d \neq 0$, $N(UU^+) = 1$, we have that

$$x'_0 = (aa^* + dd^*)\ x_0 + x_3\ (ad^* + a^*d)$$
$$x'_1 = (aa^* - dd^*)\ x_1 + ix_2\ (ad^* - a^*d)$$
$$x'_2 = (aa^* - dd^*)\ x_2 + ix_1\ (a^*d - ad^*)$$
$$x'_3 = (aa^* + dd^*)\ x_3 + x_0\ (a^*d + ad^*\ ) \qquad (3.5)$$

with $I = U\,U^+ = U^+U = \begin{pmatrix} aa^* + dd^* + a^*d + ad^* & 0 \\ 0 & aa^* + dd^* - a^*d - ad^* \end{pmatrix}$. (3.6)

In order to have $N(I) = N(U\,U^+) = 1$, let us consider that

$$aa^* + dd^* + a^*d + ad^* = \lambda \equiv \lambda(....)$$
$$aa^* + dd^* - a^*d - ad^* = \lambda^{-1} \qquad (3.7)$$

so that $N(I) = N(U\,U^+) = 1\ \forall\lambda$ except $\lambda = 0$.

Let us consider now the particular case in which *a* and *d* are real numbers. Owing to (3.7), we obtain that $(a^2 - d^2) = 1$; $\lambda = (a + d)^2$; $\lambda^{-1} = (a - d)^2$, and (3.5) assume the following final form

$$x'_0 = (a^2 + d^2)\ x_0 + 2adx_3 = \frac{\lambda + \lambda^{-1}}{2}\ x_0 + \frac{\lambda - \lambda^{-1}}{2} x_3\ ;$$
$$x'_1 = x_1;\quad x'_2 = x_2;$$
$$x'_3 = (a^2 + d^2)\ x_3 + 2ad\ x_0 = \frac{\lambda + \lambda^{-1}}{2}\ x_3 + \frac{\lambda - \lambda^{-1}}{2}\ x_0\ . \qquad (3.8)$$

Equation (3.8) represents the Lorentz transformations of special relativity for $\lambda = \left(\frac{c+v}{c-v}\right)^{1/2}$ and $\lambda^{-1} = \left(\frac{c-v}{c+v}\right)^{1/2}$, c being the velocity of the light in vacuum. Thus, in conclusion, this theory may be derived by the biquaternions.

There is an additional and important result. Using the biquaternions, we may obtain a generalization of the relativistic invariant of special relativity. Again consider Equation (3.4) where we now admit that *a* and *d* are complex numbers, and $b = c = 0$. Using the corresponding LHBT(*Z*), we express *Z'* in the following form

$$Z' = b_0x_0 + b_1x_1e_1 + b_2x_2e_2 + b_3x_3e_3 \qquad (3.9)$$

where the norm of *Z'*, *N(Z')*, is no longer $(x_0^2 - x_1^2 - x_2^2 - x_3^2)$ but its generalization

$$N(Z') = x_0\, b_0^2\, x_0 - x_1 b_1^2\, x_1 - x_2\, b_2^2 x_2 - x_3\, b_3^2 x_3\ . \qquad (3.10)$$

In this case, we have used an LHBT that enables us to pass from a particular space-time (the vacuum) to a generalized space-time. This space time seems to have physical properties characterized by characteristic functions $b_\mu = b_\mu(...)$. Thus, different space-times are possible, according to the biquaternion algebra. They are characterized by different characteristic functions $b_\mu = b_\mu(...)$. These space-times should have generalized relativistic invariants given by (3.10). Such different space-times should have total anisotropy.

According to Reference 2, we have obtained a generalization of the usual space-time only using biquaternions. The problem of establishing the actual existence of such space-times that admit the generalized invariants should be discussed and investigated in detail. We have to consider, in fact, that Minkowski space and special relativity were specifically conceived for motion in vacuum, intended as an empty space. Minkowski's space provides a geometrization of the homogeneity and isotropy of this empty space. *The validity of the usual relativistic invariant for electromagnetic interactions is strongly established by a considerable number of experimental confirmations. Its validity is restricted, however, to point-like particles moving in empty space under electromagnetic interactions.* The generalization introduced by Equation (3.10) states that we may have physical cases in which the particles cannot be approximated as massive points, and their motion does not generally occur in empty space, but in media properly characterized by the characteristic functions b_μ which delineate the features of inhomogeneity and anisotropy of the media under consideration. On the matter of a general consideration regarding Equation (3.10), we have to remember here that E. Fermi,[8] as early as in 1949, advanced doubts on the validity of considering the usual concepts of geometry for the small regions regarding strongly interacting particles by nuclear forces. In addition, phenomenological data are available on the behavior of the mean life of unstable hadrons with energy. A variance is expected with the predictions of special relativity.[9]

The introduced generalization of Minkowski space-time of the usual relativity immediately leads to a modification of the standard equations of the traditional theory. Instead of m_0c^2 of the usual formulation, we must consider now $m_0c^2b_0^2$ as the mass of a particle entering in a physical medium that has a generalized invariant as indicated in (3.10). In the case of the electron entering the physical medium of the

New Energy Times Archive

proton, we have to consider its mass m_e renormalized as $m_0c^2b_0^2$.

AN APPLICATION OF THE GENERALIZED SCHRÖDINGER'S EQUATION TO EXPLAIN THE FORMATION OF THE NEUTRON AS COMPRESSED HYDROGEN ATOM

Consider the formation of the system (p, e^-). We may use Equation (2.7) with $V(r) = -e^2/r$ and $V_H(r)$ given in (2.8). Equation (2.7) is rewritten as follows

$$\left(-\frac{\hbar^2}{2m}\nabla^2 - \frac{e^2}{r} - V_0\frac{e^{-br}}{1-e^{-br}}\right)\psi = E\psi \tag{4.1}$$

where we have that

$$V(r) = -\frac{e^2}{r} \cong -e^2 b\frac{e^{-br}}{1-e^{-br}} \quad \text{for } r \to 0\,. \tag{4.2}$$

Our generalized Schrödinger's equation is rewritten as

$$\left(-\frac{\hbar^2}{2m}\nabla^2 - V_0'\frac{e^{-br}}{1-e^{-br}}\right)\psi = E\psi \tag{4.3}$$

with $V_0' = e^2 b + V_0$.

In the standard radial form, introducing the dimensionless variable $x = br$, we may have that

$$\frac{d^2\varphi}{dx^2} + \left(\beta^2\frac{e^{-x}}{1-e^{-x}} - \alpha^2\right)\varphi = 0 \tag{4.4}$$

with $\varphi = r\,\psi(r)$ and

$$\beta^2 = \frac{2mV_0'}{\hbar^2 b^2} \quad ; \quad \alpha^2 = \frac{-2mE}{\hbar^2 b^2} \tag{4.5}$$

$\beta > 0$, $\alpha > 0$ (see Reference 7 for details).

Note that it must be $\alpha = (\beta^2 - n^2)/2n$ and $\alpha > 0$, $n = 1, 2, \ldots$. The binding energy of the (p, e^-) system is given by

$$E_n = -V_0'\left(\frac{\beta^2 - n^2}{2n\beta}\right)^2 \tag{4.6}$$

For $n = 1$ it gives

$$E_1 = -\frac{\hbar^2 b^2}{8m}\left(\frac{2mV_0'}{\hbar^2 b^2} - 1\right)^2 \quad . \tag{4.7}$$

Let us analyze if the bound state (p, e^-) is possible. Consider the renormalization of the electron mass. As previously established, we have that $m = m_0c^2b_0^2 = 0.511b_0^2$ MeV = 1.306 MeV according to the possible value $b_0 = 1.65$ given in Reference 9.

Let us examine now the possibility of bound state formation for (p, e^-). Our system (p, e^-) may be considered at the rest mass for the proton, m_p= 938.279 MeV. The renormalized mass of the electron has been calculated to be m = 1.306 MeV. Thus, the (p, e^-) system has the following rest energy:

(p, e^-) system : (938.279 + 1.306 - 0.072) MeV = 939.513 MeV.

This is just the rest energy of the neutron. Thus, the neutron may be formed as a compressed hydrogen atom with the electron penetrating inside the dense medium of the proton p.

THE SPIN OF THE NEUTRON

Proton, electron and neutron have spin $\pm\,1/2\hbar$. Consequently, it could appear problematic to suggest here the fusion of the proton with the electron giving the neutron that has $\pm\,1/2\hbar$ as value of the spin. In this regard we have to outline here two important results that here previously obtained by E. Rutherford[1] and by A.M. Dirac[10] in 1971. As previously stated, Rutherford conceived his hypothesis on the neutron as a "compressed hydrogen atom." We must consider the initiation of Rutherford's compression of the electron inside the proton with the electron that can only penetrate inside the proton with the relative spin in phase and with its angular momentum parallel to the spin of the proton. Only singlet states are formed. When the electron is compressed in the proton, the angular momentum of the electron coincides with the spin of the proton and the spin of the formed neutron coincides with the spin of the proton.

In 1971, Dirac introduced a new relativistic equation for particles of non-zero rest-mass, allowing only positive values for the energy. There is great formal similarity between it and the usual relativistic wave equation for the electron, but the physical significance is very different. In particular, the new equation gives integral values ($n = 0, 1, 2, \ldots$) for the spin as possible.

For the electron spin penetrating inside the proton we may conceive the usual spin $S_x = \frac{1}{2}\hbar E_{01}$, $S_y = \frac{1}{2}\hbar E_{02}$, $S_z = \frac{1}{2}\hbar E_{03}$ with E_{01}, E_{02}, E_{03} biquaternions given in the following manner

$$E_{01} = \begin{pmatrix} 0 & 1 & 0 & 0 \\ 1 & 0 & 0 & 0 \\ 0 & 0 & 0 & 1 \\ 0 & 0 & 1 & 0 \end{pmatrix} ; \quad E_{02} = \begin{pmatrix} 0 & -i & 0 & 0 \\ i & 0 & 0 & 0 \\ 0 & 0 & 0 & -i \\ 0 & 0 & i & 0 \end{pmatrix} ; \quad E_{03} = \begin{pmatrix} 1 & 0 & 0 & 0 \\ 0 & -1 & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & -1 \end{pmatrix} ; \tag{5.1}$$

and consider the transformation $E_{0i}' = UE_{0i}U^+$ as stated to generalize Schrödinger's equation. In this manner we obtain that

$$E_{01}' = 4S^2E_{01} \; ; \quad E_{02}' = 4S^2E_{03} \; ; \quad E_{03}' = -4S^2E_{02} \quad . \tag{5.2}$$

The spin, penetrating the electron inside the proton, becomes

$$S_x = \frac{1}{2}\hbar E_{01}' ; \quad S_y = \frac{1}{2}\hbar E_{02}' ; \quad S_z = \frac{1}{2}\hbar E_{03}' \quad . \tag{5.3}$$

The orbital angular momentum of the electron is analogously expressed by the biquaternions

$$\frac{1}{\hbar}4S_1^2L_1 \; ; \quad \frac{1}{\hbar}4S_1^2L_2 \; ; \quad \frac{1}{\hbar}4S_1^2L_3 \; . \tag{5.4}$$

The total angular momentum of the electron penetrating the proton is the sum of the orbital angular momentum and spin

$$J_1 = \frac{1}{\hbar}4S_1^2L_1 + \frac{1}{2}\hbar E_{01}'$$

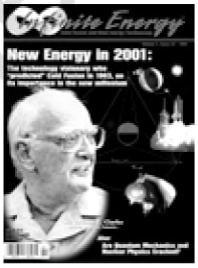
$$J_2 = \frac{1}{\hbar}4S_1^2L_2 + \frac{1}{2}\hbar E_{02}'$$

$$J_3 = \frac{1}{\hbar}4S_1^2L_3 + \frac{1}{2}\hbar E_{03}' \tag{5.5}$$

that may now give $J_1 = J_2 = J_3 = 0$. In this case the spin of the formed neutron coincides with the spin of the proton.

CONCLUSIONS

In this paper we have given a theoretical indication of the possibility of the cold fusion of protons and electrons as a natural bound state of particles at low temperatures. This possibility must be dated to 1920, when Rutherford introduced his conception of the neutron as a compressed hydrogen atom. Rutherford's conception of the neutron was abandoned, because a number of incompatibilities with quantum mechanics, such as the need of "positive" binding energy (m_p = 938.27 MeV, m_n = 939.57 MeV, m_e = 0.5 MeV), the inability to account for the total spin 1/2, and, finally, the impossibility of recovering the relatively large mean life of the neutron with regard to the small electron mass. The reason that it is impossible for quantum mechanics to agree with Rutherford's model of the neutron resides in the limited attitude of such traditional quantum theory to connect physical phenomena outside the sphere of the atomic processes. Quantum mechanics was conceived for the atomic structure of matter. At this stage of the physical investigation, the electron can indeed be considered as being point-like and moving in a homogeneous and isotropic vacuum. Under these conditions we have the full validity of traditional quantum mechanics, as also confirmed by a large amount



GIVE AN *INFINITE ENERGY* SUBSCRIPTION TO THE COLLEGE LIBRARY OF YOUR CHOICE.

New Energy Times Archive

of experimental results. However, in the case of the neutron model of Rutherford, we have physical conditions that are very different from the usual conditions of atomic matter. Here, the electron must be compressed into the proton. In order to explain the total immersion of the wave packet of the electron inside the dense medium of the proton, we have to reformulate and to generalize the old quantum mechanics. This is, of course, the physical significance of introducing Equation (2.7) that represents a generalization of the usual Schrödinger's equation.

There are specific reasons that require a generalization of Schrödinger equation as it has been obtained by us using biquaternions. Electromagnetic interactions are long range, occurring at distances greater than the charge radius of the particles. For this reason, such particles can be actually approximated as being pointlike. In this manner, long-range electromagnetic interactions occur among isolated points that experience action at a distance and admit potential functions. This was, of course, the original formulation as it was conceived by Galileo, Newton, and Einstein.

The presence of strong interactions, instead, modifies profoundly the whole view of our traditional conceptions. They are established experimental facts that the charge radius of hadrons does not increase with mass, and it coincides with the range of the strong interactions. Thus, in accord with the generalized Schrödinger's equation obtained in (2.7), strong interactions characterize the hadrons that may be in a physical state of mutual penetration or overlapping of their charge volumes. If it is so, in this case such particles cannot be actually approximated as pointlike.

This is a result that delineates a new profile for the interactions existing in Nature. We need a generalized Scrhödinger equation possessing the attitude to describe simultaneously the local features and the interactions to all the points of overlapping during the standard interaction between two particles. This is precisely the duty of our generalized Scrhödinger's equation discussed in the previous sections.

We have interactions that have two components: the local part is expected to represent the electromagnetic interactions, or, generally speaking, some other features of strong interactions, and we have also a non local component representing the effect of wave overlapping. In analogy with the whole view of the physics, we have to acknowledge here that non-local interactions resulting from wave overlapping, are in general of non-potential kind, as clearly obtained in the previous sections of this paper.

In conclusion, if we have a local physical system, we have only potential forces; the particles can be approximate as pointlike and they admit only action at a distance. There are other physical conditions in which we need to represent the extended features of physical objects that are in contact. In this case we have a non-potential character of non-local forces, and it is representative of the extended character of the wave packets that are involved. The extended character of the physical objects requires a physical treatment in which is involved the generalization of Schrödinger's equation on the one hand and a generalization of the usual invariant of relatively on the other hand. This is precisely the case of the possible cold fusion of the proton and the electron into a neutron. From an experimental view point it must be clear that the formation of such a bound state $(p, e^-) \rightarrow n$ may be obtained if and only if we realize experimentally the deliberate combination of protons and electrons in a situation that promotes wave function overlap.

We may outline here some other features that characterize the cold fusion of (p, e^-) in a neutron. The central mechanism of the cold fusion of particles is the deep mutual penetration of the wavepackets at mutual distances equal or smaller than 1 F. This implies the dominance of non-local interactions as established by the generalized Schrödinger's equation.

We may now complete this section returning briefly to the problem of the spin of the formed neutron, showing that our solution of the previous pages is in accord with the solution that was indicated by Dirac. On the basis of Equation (10), we consider the following biquaternions

$$\beta = iE_{20}\,; \quad \alpha_1 = -E_{13}\,; \quad \alpha_2 = E_{11}\,; \quad a_3 = E_{30}\,;$$

Dirac introduced the following equation

$$(a_\mu \delta^\mu + \beta)\, q\, \psi = 0$$

with q a column vector having components $q = (q_1, p_1, q_2, p_2)$ and ψ a four-component wavefunction. The biquaternions a_i $(i = 1, 2, 3)$ verify the following property

$$a_\mu \beta\alpha_\nu + \alpha_\nu \beta\alpha_\mu = 2\,\beta\,\eta_{\mu\nu}$$

The spin connected to the previous equation was obtained to be

$$S_{ij} = -(a_i \beta\alpha_j - \alpha_j \beta\alpha_i)\, q\, q^T / 8$$

with q^T a numerical value. Dirac found that

with $$s^2 = s_{12}^2 + s_{23}^2 + s_{31}^2 = (q_1^2 + p_1^2 + q_2^2 + p_2^2)^2 / 8 - 1/4 = s(s+1)$$

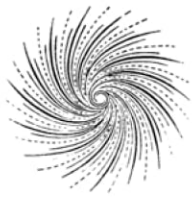
$$s = (q_1^2 + p_1^2 + q_2^2 + p_2^2)^2 / 4 - 1/2 = (n + n')/2$$

and $n + n' = 0, 1, 2, \ldots\ldots \qquad s = 0, 1/2, , 1, \ldots.$

It is so seen that the value $s = 0$ is for the ground state. q_k and p_k $(k = 1, 2)$ were considered to be the quantum numbers of two oscillators.

The analogy between Dirac's derivation and our previous formulation is very indicative. We intend to examine now the case of the d-d reaction. Consider the general case of three particles having masses m_1, m_2, m_3, respectively. Let us admit that m_1 and m_2 interact by a repulsive Coulomb potential $V(r) = +e^2/r$ while, instead, the Coulomb interaction between m_1 and m_3 and between m_2 and m_3 is attractive. We have to introduce here a preliminary quantification of the overlap effects that are induced by the Coulomb attractive interac-

The **Planetary Association for Clean Energy**, Inc.



Since 1975 ...
...a collaborative network of advanced scientific thinking

advanced therapeutics
Brown's Gas
consciousness studies
electromagnetic field bioeffects
free energy devices
human rights of scientists
legacy of Nikola Tesla
nuclear and toxic waste decontamination
scalars & the fundamentals of physics

Ask for a sample copy of our Newsletter with a book, video, software & CD-ROM list.

The **Planetary Association for Clean Energy**, Inc.
100 Bronson Avenue, Suite 1001,
OTTAWA, Ontario K1R 6G8 Canada
(613) 236-6265; fax: **(613) 235-5876**

New Energy Times Archive

tion. Let us consider the system (m_1, m_3). Using the generalized Schrödinger's equation discussed in detail in the previous sections, we may establish that the cumulative interaction characterizing such system may be expressed as it follows

$$V_H(r) = (-e^2 b - E_0 Q)\frac{\exp(-br)}{1-\exp(-br)} = V_1(r) + V_2(r) \tag{6.1}$$

with

$$u(r) \cong c_1 \exp(-br); \qquad c_1 = \frac{b^{3/2}}{\pi^{1/2}} \tag{6.2}$$

$$\psi(r) \cong c_2 \frac{\exp(-\alpha r)}{r}[1 - \exp(-br)]$$

where we are considering the space region in which $\frac{\exp(-\alpha r)}{r} \cong b$. Q is the overlap integral of the wavefunctions, and it results to be

$$Q \cong \left(\frac{1}{R^3}\right)\left[\exp[-(\alpha+b)r]\frac{r}{\alpha+b} - \exp[-(\alpha+2b)r]\frac{r}{\alpha+2b}\right] \tag{6.3}$$

with $R \cong b^{-1}$.

In conclusion, Equation (6.1) may be expressed in the following manner:

$$V_1(r) = -e^2 b\frac{\exp(-br)}{1-\exp(-br)} = -\frac{e^2}{r} = -\frac{e^2}{R} \qquad \text{for } r = R$$

$$V_2(r) = -E_0 Q\frac{\exp(-br)}{1-\exp(-br)} = -\frac{E_0 Q}{br} = -\frac{e^2}{r}\left(\frac{r}{2R} - \frac{r^3}{R^3}\right) = -\frac{e^2}{2R} + \frac{e^2 r^2}{2R^3} \tag{6.4}$$

where $\frac{E_0}{b} \cong e^2$

We obtain that

$$V_H(r) = V_1(r) + V_2(r) = -\frac{3}{2}\frac{e^2}{R} + \frac{e^2 r^2}{2R^3} \tag{6.5}$$

where $V_H(r)$ accounts for the attractive Coulomb potential between m_1 and m_3 and for the contributions deriving from the overlap of the wave packets. If we include also the Coulomb repulsive interaction acting on the mass m_1 we have the final expression

$$V(r) = \frac{e^2}{r} - \frac{3}{2}\frac{e^2}{R} + \frac{e^2 r^2}{2R^3} = \frac{e^2}{r} - \frac{e^2}{2R^3}\left(3R^2 - r^2\right) \tag{6.6}$$

Let us discuss Equation (6.6) in the case of a possible d-d reaction in the phenomenologies regarding cold fusion. As we know, a Pd metal lattice can confine a large quantity of hydrogen in itself, *i.e.* about 1000 times its own volume. The basic key is that deuterium/hydrogen molecules are dissociated into atoms and are ionized to bare nuclei. There are several detailed results to this regard.[11] We consider deuterium to be ionized. As a consequence, in this case, the use of the Fermi-Thomas potential is no longer justified while, instead, it was used in previous studies. To a first approximation, we can treat the electrons as completely free to, on the average, spread throughout an entire volume with a uniform density ρ in a physical condition that promotes wave function overlap as indicated in (6.1) and (6.3). It is evident that this model tends to underestimate rather than overestimate the electron effect on barrier penetration for d-d reaction. In conclusion, we may establish the following model: a symmetric distribution of deuterium nuclei on which is superimposed a uniform distribution of negative charge such that the total system is neutral. We may then divide the system into atomic volumes that we may assume to be spherical in shape, and each with a deuterium nucleus at its center. The radius of the atomic volume is denoted by R, and the uniform electron density may be determined by $\frac{4}{3}\pi R^3 \rho = 1$. The Coulomb interaction potential due to the nuclei alone, results to be $V_n(r) = +\frac{e^2}{r}$ with $r_1 < r < R$ where r_1 is the nuclear radius of the deuterium. If one performs calculations for the potential based on the above uniform electron-density model, one finds that it is given by

$$V(r) = \frac{e^2}{r} - \frac{e^2}{2R^3}\left(3R^2 - r^2\right) \qquad \text{for } r_1 < r < R. \tag{6.7}$$

That is just the cumulative potential that we found in (6.6) applying the generalized Schrödinger's equation containing the contributions from the overlap of the wave packets. Equation (6.7), obtained on the basis of a classical model, furnishes direct evidence of the correctness of all the theoretical elaboration. Equation (6.6) is then the potential that we must use in order to calculate the barrier penetrability. We have that

$$P = \exp\left[\frac{-2(2m)^{1/2}}{\hbar}\int_{r_1}^{r_3}\left[V(r) - E\right]^{1/2} dr\right] \tag{6.8}$$

with new limits

$$\frac{e^2}{r_2} = E \quad \text{, unmodified potential}$$

$$\frac{e^2}{r_3} - \left(\frac{e^2}{2R^3}\right)\left(3R^2 - r_3^2\right) = E, \quad \text{modified potential by overlap} \tag{6.9}$$

For the case with overlap electron effects, we must evaluate (6.8) using (6.6) between the limits r_1 and r_3. An approximate calculation may give immediately the ratio of the modified, P^*, to unmodified, P, barrier penetrability

$$\frac{P^*}{P} = \exp\left[\frac{-2(2m)^{1/2}}{\hbar}\int_{r_1}^{r_3}\left[V(r) - E\right]^{1/2} dr\right] = \exp\left[f(E, r_2) - f(E, r_3)\right] \tag{6.10}$$

with

$$f(E, r) = \frac{2(2m)^{1/2}}{\hbar}\frac{e^2}{(E)^{1/2}}\left[\cos^{-1}\left(x^{1/2}\right) - x^{1/2}(1-x)^{1/2}\right]$$

and $x = \frac{Er}{e^2}$

The effect of the overlap of electrons on deuterium barrier penetrability is found speedily. As an example, if we consider the radius of the atomic volume to be $R = 1.78\text{x}10^{-8}$ cm, we obtain values of P^*/P equal to 5.2 for $E = 100$ eV and equal to $4.9\text{x}10^{10}$ for $E = 20$ eV. The relevance of the contribution is rather evident. It confirms the possibility of cold fusion phenomenologies, and, in particular, to have nuclear reactions at low energies.[12]

References

1. E. Rutherford, *Proc. Roy. Soc.* A97, 374, 1920.
2. E. Conte, *Physics Essays* 1, 70, 1992; 6, 532, 1993; 7, 429, 1994; 8, 51, 1995; 8, 605, 1995; 9, 4, 1996; 10, 511, 1997; *Hadronic J.,* 16 (4), 261, 1993; *Biquaternion Quantum Mechanics,* Pitagora Editrice; Bologna, Italy, 1996.
3. T. Phipps, *Phys. Rev.* 118, 1653, 1960.
4. N. Gisin, 1982 Dept. de Physique Teorique, Université de Géneve.
5. M. Kostin, *J. Chem. Phys.* 57, 3589, 1982;
E. Kanai, *Progr. Theor. Phys.* 3, 440, 1948.
6. P. Caldirola, E. Montaldi, *Nuovo Cimento* 53B, 291, 1979
R.M. Santilli, *Hadronic J.* 2, 6, 1979; 14, 459, 1991.
7. S. Flugge, *Practical Quantum Mechanics,* Springer-Verlag, 1974.
8. E. Fermi, *Nuclear Physics,* Univ. Chicago Press, 1947.
9. F. Cardone, R. Mignani, R.M. Santilli, *J. Phys.* 418, L141, 1992.
10. P.A.M. Dirac, *Proc. Roy. Soc.* A322, 435, 1971.
11. F. Antonangeli, A. Balzarotti, A. Bianconi, P. Perfetti, *Nuovo Cimento* B, 39, 704, 1977.
12. T. Mizuno, T. Ohmori, M. Enyo, *Proc. Int. Conf. Low Energy Nuclear Reactions,* College Station, Texas, Sept. 13-14, 1996.

The Author

Dr. Elio Conte, born in Italy in 1946 and PhD in 1970, is Professor of Theoretical Physics and Director of the Center for Studies on Radioactivity-free University Institute founded in Bari, Italy in 1985. He held teaching positions at the Advanced Institute of Cybernetics in San Marino, Republic of San Marino for over three years, and at the post-graduate school of the University of Bari for over ten years, where he also directed various laboratories of scientific research. He is author of over 120 research papers and some advanced monographs in theoretical and nuclear physics. Currently, in theoretical physics, his interest has concerned the foundations of quantum mechanics and the possible generalization of such quantum theory; in nuclear physics his activity

New Energy Times Archive

has concerned various fields, and, in particular, the weak interactions, nuclear reaction mechanisms, radioactivity and interactions of radiation with matter.

Experimental Evidence for the Cold Fusion of Protons and Electrons into Neutrons

by E. Conte, M. Pieralice
Centro Studi Radioattività - Bari - Italy
ISOI - C.N.R. - Bari - Italy
[C.N.R. Grant n. 95.1586.CT11]

Rather recently[1] we obtained a generalization of Schrödinger's equation. Such a formulation was evidence that we need to review some conceptual features when we consider the interactions existing in nature. Electromagnetic interactions are long range; they occur at distances much greater than the charge radius of the particles. Under these conditions, the particles may actually be approximated as being pointlike. They only experience action at a distance. Generally speaking, when a physical system admits only potential forces, its constituents are pointlike and admit only action at a distance. When we consider physical conditions in which the particles interact with contact effects, the inappropriateness of the pointlike approximation emerges. In this case a generalized Schrödinger's equation is required for a description. It is evident that the interaction now possesses two components, the local component, previously considered, plus an integral component representing the interaction of overlapping of the wavepackets of the particles involved in the interaction. We may say equivalently that such non-potential interaction is representative of the extended structure of the particles considered.

This is the innovative character of the generalized Schrödinger equation. We applied it to the case of a proton and an electron interacting via the usual Coulomb potential $V(r)=-e^2/r$, and we found that the overlapping interaction of two such particles should enable the formation of the neutron as a compressed system—consisting of the proton and the electron. In effect, the theoretical formulation predicts that owing to the overlapping of the wave packets, the electron should penetrate inside the proton.

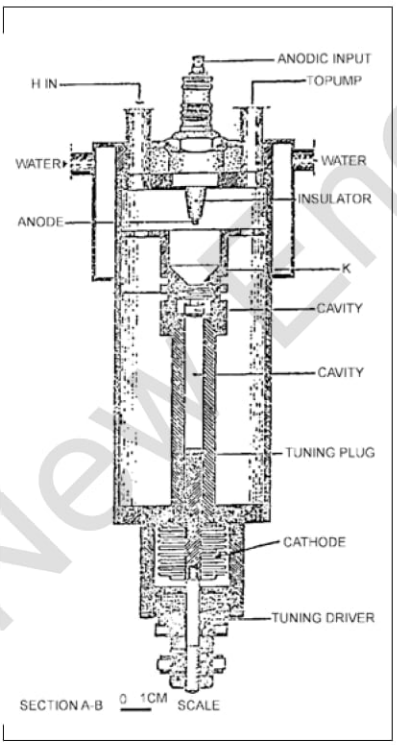


Figure 1. One of the klystrons built for the experiment.

The cold fusion of protons and electrons, obtained here as an expected natural bound state of particles at low temperatures, was first suggested by Rutherford in 1920 [2]. He considered the neutron as a compressed hydrogen atom. The existence of the neutron was subsequently confirmed in 1932 by Chadwick,[3] but the Rutherford conception of such a particle was rejected, because of a number of incompatibilities with quantum mechanics. There was the problem of the binding energy for m_n = 939.57 MeV; m_p = 938.27 MeV; m_e = 0.51 MeV; the impossibility of explaining the total spin 1/2 of the neutron from two components each of spin 1/2 was evident, as well as the impossibility of accounting for the relatively large mean life of the neutron.

In our previous papers[1] we showed that such incompatibilities are overcome and resolved in the framework of our quantum mechanical generalization. In effect, we have to consider that quantum mechanics was originally conceived for the analysis of atomic structures. In this case the electron may actually be considered to be point-like and moving in the homogeneous and isotropic vacuum. These are the actual physical conditions that require the exact applicability of the local-potential structure of quantum mechanics. The physical conditions regarding Rutherford's compression, instead, are different from those characterizing the atomic structure. In order to have the total immersion of the wavepacket of the electron inside the dense medium of the proton, we need to consider non-local interactions due to the overlapping of the wavepackets.

It is evident that such new expectations of a generalized quantum theory require experimental confirmations. In this regard, an experiment was performed to analyze the possibility of cold fusion of protons and electrons into neutrons. Hydrogen was obtained by electrolysis (water + H_2SO_4), dried on silica , and filtered. The possible presence of deuterium was considered to be only that obtainable by such a process, and it was evaluated to be not more than one-tenth of the natural abundance. Hydrogen at a pressure about 0.1 Torr was introduced in an oscillator (Figure 1) having brass walls 0.2 cm thick, with cavities of stainless steel having also a 0.2 cm thickness. The size of the whole oscillator was 15 cm long with a 5 cm diameter. A cooling water system was considered, but the possibility of particles crossing through the water was eliminated. The device we used worked as a klystron with an anode-cathode (grounded) potential difference of about 500 Volts, and generating a microwave electric field. This field succeeded in maintaining a cold plasma, a considerable number of protons mixed and colliding with an equal number of free electrons for a time larger than 10^{-8} sec. This limit was suggested by the known average recombination time of the ionized hydrogen atom. The high frequency was fixed to be of the order of 10^{10} sec^{-1}, inducing ionization in the low-pressure pure hydrogen contained in the resonant cavities of our klystron-like oscillator.

The expected effect was that, because of the microwave electric field, there would be a very great number of collisions between free protons and free electrons. We obtained a deliberate combination of protons and electrons in a physical situation that promoted wavefunction overlap. In the event that they were formed, the neutrons could pass through the walls of the oscillator. In this regard, a number of samples of different elements were placed outside the walls in order to verify the presence of neutrons by activation analysis. A variant was also used by detecting neutrons with thermoluminescence using $Li_2B_4O_7$ TLD dosimeters for the detection of thermal neutrons. The dosimeters were read by using thermoluminescence instrument Harshaw 3000 TLD.

Concerning the neutron activation, we used samples of ^{55}Mn (giving ^{56}Mn with $T_{1/2}$= 2.576 hours and γ-emission at 0.847 MeV), ^{63}Cu (giving ^{64}Cu with $T_{1/2}$ = 12.900 hours and γ-emission at 0.656 MeV), and ^{197}Au (giving ^{198}Au with $T_{1/2}$ = 2.693

New Energy Times Archive

days and γ-emission at 0.412 - 0.676 - 1.088 MeV). Gamma spectrometry was performed by using a Canberra coaxial Ge-detector having 13% relative efficiency, interfaced with Canberra multichannel analyzer S.20.

All the measurements, performed after six hours of irradiation, showed that neutrons were actually formed. Activities about (5 - 6) Bq were measured for the activated samples. Measurements were performed with a standard error of about 10%. On the basis of the relation

$$\Phi = \frac{A}{60hWs_a}$$

with A= dis/min; $h=10^{-3}$ (efficiency of the germanium detector); $W = 10^{-2}$ (solid angle); Sa @ 1 cm^{-1} (macroscopic cross section); the flux Φ of the neutrons was estimated to be about 10^5 n/sec.

Results in accord with this evaluation were also obtained by using thermoluminescence. In conclusion, it seems that the neutron was actually formed during the experiment.

It must be stated here that our experiment only confirmed previous results that were obtained in 1960.[4] In particular, we must express here that the particular purpose of this present work was to give an exact reproduction of the previous experiment that in 1960 was performed by these authors.[4]

References

1. E. Conte, *Physics Essays* 1, 70, 1992; 6, 532, 1993; 7, 429, 1994; 8, 51, 1995; 8, 605, 1995; 9, 4, 1996; 10, 511, 1997; *Hadronic J.*, 16 (4), 261, 1993, "A Re-interpretation of BCS-theory in superconductivity by Using Biquaternion Quantum Mechanics," Submitted for publication in *Physics Essays* (authors: E. Conte and M. Pieralice), "On a Re-examination of the EPR Argument and Bell's Inequality by Using the Biquaternion Calculus in Quantum Mechanics: The First Explicit and Concrete Case of Hidden Variable." Submitted for publication on *Physics Essays*, For a detailed exposition regarding the Biquaternion Quantum Mechanical foundations of this experience, and for an historical account of previous works regarding the possibility of formation of the neutron by proton and electron, see: "Theoretical Indications of the Possibility of Obtaining Nuclear Reactions at Low Energy" (and References 3,4,5,6, here quoted) that appears in Volume 24 of *Infinite Energy*, 1999. "Biquaternion Quantum Mechanics," Pitagora Editrice; Bologna, Italy, 1996.

2. E. Rutherford, *Proc. Roy. Soc.* A97, 374, 1920.

3. J. Chadwick, *Proc. Roy. Soc.* A136, 374, 1932.

4. C. Borghi, C. Giori, A. Dall'Olio, *Comunicacoes do CENUFPE*, n 8, Recife, Brazil; Collected works of G. Borghi, Andromeda Editor, Bologna, Italy, 1969; C. Giori, "On the Emission of Neutrons from Cold Hydrogen Plasma" (private communications).

Letter to Editor from Ruggero Maria Santilli [January 27, 1999]
Institute for Basic Research P.O.Box 1577,
Palm Harbor, Florida, 34682, USA, ibr@gte.net

In my worldwide search for colleagues interested in testing the possibility of fabricating the neutron in the laboratory, I contacted Elio Conte from Bari, Italy, in early 1990 and provided him with the totality of information available to me at that time, including complimentary copies of numerous papers and books.

I feel obliged to report the following, what I believe to be intentional, documented misrepresentations in a recent note by Elio Conte and Maria Pieralice in *Infinite Energy* Vol. 4, Issue No. 23, pages 67-68, 1999, on the apparent "cold fusion" of protons and electrons into neutrons plus neutrinos. These misrepresentations were evidently unknown to the Editorial Board of *IE* at the time of the publication of the note:

1) The authors have intentionally avoided reference to the first experiment on the synthesis of neutrons via protons and electrons, performed by Don Borghi *et al.*,[1] for the evident intent of attempting to secure the first experimental priority on the issue via unethical means. The case is rendered particularly grave by the fact that, as documented and well known to Elio Conte, the late Don Borghi was an Italian priest and physicist from the University of Milano who experienced immense difficulties and obstructions which prevented the carrying out of the test in Italy. In fact, the test was eventually conducted in Recife, Brazil. It is important to clarify at IE that, even though evidently preliminary, with various insufficiencies and in need of verification, the first known scientific priority for the laboratory synthesis of the neutrons from protons and electrons belongs to Don Borghi and his team.

2) The authors have intentionally avoided the quotation of at least some of the vast literature on the new hadronic mechanics which has been constructed primarily for the quantitative representation of the laboratory synthesis of the neutron from protons and electrons (see a series of articles on the new mechanics that I have been serializing for *IE*). The evident intent, this time, was that of attempting to secure the first theoretical priority via unethical means. For instance, statements such as that at the bottom of p. 67, *loc. Cit.*, "...we developed a theory (2) ... via a non-local, non-potential interactions due to the mutual overlapping of the wavepackets [of the proton and electron]" are copied ad litteram from the foundations of hadronic mechanics dating back to 1978, whose knowledge by Elio Conte is documented beyond doubt. It is important to clarify at *IE* that the hypothesis on the neutron as a bound state of a proton and an electron has been studied by numerous physicists throughout this century. However, to my best knowledge following decades of studies of the problem, none of these studies achieved a consistent model because of truly sizable technical difficulties for all attempts based on quantum mechanics or generalized non-invariant theories, such as: inability to represent the spin of the neutron (because the spin 1/2 of the neutron cannot be reached via two constituents each with spin 1/2); the inability to represent the total rest energy of the neutron because it would require a positive binding energy (an anathema in physics because the sum of the rest energies of the constituents is smaller than the rest energy of the neutron); the inability to represent the anomalous magnetic moment of the neutron (because it is simply unreachable via the magnetic moments of protons and electrons); the inability to represent the size and mean-lifetime of the neutron; and other major technical difficulties. The first invariant resolution of the above problems, thanks to the use of hadronic mechanics and the first exact-numerical representation of all characteristics of the neutron as a bound state of a proton and an electron, was achieved by Santilli in 1990,[2] as is well known to Conte.

3) Conte and Pieralice intentionally abstain from referencing the lack of invariance of their biquaternionic theory, with consequential lack of physical value. As a specific illustration, if a certain physical characteristic is predicted by the biquaternion theory at a given time (say, the fifteen minute mean-lifetime of the neutron), the same characteristic has a different numerical value at a later time (say, the mean-lifetime can become one hour—for details on the problems caused by non-invariance see the forthcoming remaining sections of Paper II (in press at *IE*). I personally conveyed to Elio Conte this major inconsistency, in my capacity as Editor of the *Hadronic Journal*, and following the submission of articles in biquaternion theories at that journal.

The above misbehaviors have caused serious damage to the ongoing efforts to establish new energies. Due to the unsettled nature of the field because of its novelty, the credibility of new experimental and theoretical results is primarily based on the ethical standards of the authors. Quite honestly, I cannot believe in the technical content of the report by Conte and Pieralice at *IE*, because of their unethical behavior in the referencing.

A final rather astonishing aspect is the fact that, while in the early 1990's we mailed to Conte in Italy numerous packages of scientific material, his answers were suspiciously restricted to a very few dry lines, to such an extent that at one point my editorial staff intervened and indicated to Conte that it was impossible to keep sending him additional scientific disclosures unless he would reciprocate with scientific disclosures of his own. He refused to disclose to us his tests on the synthesis of the neutron and we therefore stopped sending him material in the field. In retrospect, the suppression of information noted above was premeditated by Conte from day one!

Neutrons constitute one of the largest reservoirs of clean energy available to mankind. In fact, the laboratory synthesis of the neutron from protons and electrons is only the first step of the more important stimulated decay of the neutron (not quoted by Conte because he knows it is under patent and other protections) which releases electrons of about 100,000 times the energy of electrons hitting a computer screen[3] (plus the release of innocuous neutrinos), thus resulting in a clean energy in the sense of not releasing harmful radiations such as fission nuclear energy, and not leaving harmful waste.

There is little doubt that the indicated note by Conte and Pieralice constitute a major setback in this issue that is so important for society.

New Energy Times Archive

1. D. Borghi *et al.*, *Journal of Nuclear Physics*, Vol. 56, p. 147 (1993).
2. R.M. Santilli, "Apparent Consistency of Rutherford's Hypothesis of the Neutron as a Compressed Hydrogen Atom," *Hadronic J.* Vol. 13, 513 (l990); and *Chinese Journal of Systems Engineering and Electronics*, Vol. 6, 177 (1995).
3. S. Smith, "Santilli's New, Clean, Subnuclear Hadronic Energy," in *Proceedings of the International Symposium on New Energy*, M. Shaw and M. Albertson, editors (1996).

Letter received from Dr. Conte in response to the Letter to the Editor from Ruggero Maria Santilli [February 16, 1999]

Dear Dr. Mallove,

This is my response to the harsh letter that R.M. Santilli addressed to *Infinite Energy*, against me and my results. I will deliberately not engage in polemics out of respect to the Editorial Board of *Infinite Energy* and, in particular, to Dr. E. Mallove.

1) I have carried out two experimental works on the synthesis of the neutron via a proton and an electron. The first paper appeared in Issue No. 23, p. 67, 1999 of *IE* and the second work should appear in Issue 24 of *IE*. The first work did not contain any reference of the previous work of Don Borghi, C. Giori, A. Dall'Olio (designated "BGO" in what follows), since our experiment was conceived, elaborated, arranged, and performed by ourselves in a way that was absolutely independent from the original, but different than what was followed by BGO.

In particular, these authors did not aim to show the synthesis of the neutron, but instead (as it is in their paper). "to see experimentally whether there is some interaction between electric charges...and whether it may produce some kind of bound states between a proton and an electron, electrically neutral but different from a hydrogen atom state." This was the objective of BGO. They did not have a theory for the neutron and they did not expect the formation of the neutron as a consequence of a theoretical formulation, as was the case in my work. Thus, in the first of two simultaneous papers we did not quote BGO for the nature of the experiment and for the reason that now follows.

Let us consider the second experimental paper that we sent to *IE* for publication. This paper clearly indicates that R.M. Santilli was actually right writing his harsh letter! In fact, in our second experimental (simultaneous) work we were so willing "to secure the first experimental priority on the issue via unethical means" that we proposed exactly the experiment of BGO, obtaining results confirming the previous ones of BGO and explicitly writing in the paper that: "This version of the experiment was elaborated, performed, and executed by BGO in 1960." And the paper concluded: "It must be outlined here that our experiment only confirmed previous results that were obtained in 1960 by Don Borghi, Giori, Dall'Olio." It continues further: "We must express here that our particular purpose was to give an exact reproduction of the previous experiment that in 1960 was performed by these authors." This was the second Letter to the Editor that is in the hands of Dr. Mallove for publication in *IE* . Concluding, I ask to myself how is it possible that some people could still think otherwise! I reject Santilli's comments, expressed in so evident a condition of prejudice and, probably, of envy.

2.) Santilli's letter descends to a low level when he attempts to instill doubts about our results, not on the basis of scientific arguments, as one expects in a scientific journal as *IE*, but probably on the basis on his personal grudge against me. It is impossible to tolerate such a behavior!

3.) Concerning the theoretical analysis, I have derived a theoretical formulation that is now called Biquaternion Quantum Mechanics (BQM). Starting in 1990, it appeared in international journals of considerable scientific value, ones with editorial boards, referees, and so on. In the past, when it was necessary, I always quoted Santilli's previous work on the same subject. It is sufficient to read my papers to establish this fact: Santilli was always quoted when it was necessary. Thus, again, "the attempt to secure the first theoretical priority via unethical means" is an arrogant insult to me from Santilli.

Today the situation has profoundly changed. Apart from some linguistic and also conceptual aspects, the BQM has no common points with hadronic mechanics. In particular, I take the opportunity here to say that I disagree with all of Santilli's formulations, derivations, and theoretical implications. In the language, actually, I recognize some common aspects between the two formulations, but BQM has been formulated subsequently on the basis of totally independent derivations that use the biquaternions and only the biquaternions. A careful consideration of my theoretical work, also in Issue No. 24 of *IE*, also evidencing some contact points with hadronic mechanics in some formal (and only formal) developments, clearly indicates the profound differences existing between the two formulations. In particular, we have obtained a generalization of the Schrödinger equation, moving from opposite viewpoints with respect to those employed by Santilli. We have used linear homogeneous biquaternion transformations, having previously shown that all the traditional quantum mechanics is a biquaternion calculus and containing hidden variables.

I repeat: I disagree with Santilli's formulation, the same concept of overlap of the wavefunctions that we use is totally different from the conceptual background utilized by Santilli. Notwithstanding this, Santilli was always quoted when it was necessary, and also in the case of my theoretical work in Issue No. 24 of *IE*, the papers quoted in reference recall directly previous Santilli formulations. In conclusion, Dr. Santilli must convince himself that he cannot expect that all physical research runs about his ideas. He has produced his results starting in 1978, and he knows very well the scientific areas that are actually covered. Regarding my position, I do not attempt to establish or to secure any theoretical priority; I only wish to make clear here as in previous cases that I disagree with all the theoretical derivation and formulation of Santilli, and ask that my work will be considered as absolutely independent from the previous one of this author. My formulation, as frequently stated also by other authors, has the character of simplicity and clearness, and it moves in harmony with and not against all the general formulation of quantum mechanics. Thus repeating, Santilli deserves all the priority for all his ideas, and with respect to these ideas I dissent profoundly.

4.) Concerning the lack of invariance of BQM, I find it superfluous to explain here this argument. Santilli needs to understand seriously the results of Biquaternion Quantum Mechanics.

5.) Finally, I see that Santilli's letter also contains many references to the private interaction that he established with me in past years. The letter also contains many errors! I will not continue on this in order to avoid polemics and to respect *IE*. I add only a comment: Santilli spontaneously sent me his books on hadronic mechanics that I quoted when it was necessary. In sending the books and other preprints, his explicit aim was to involve me in a crusade in favor of hadronic mechanics. My answer, instead, was to publish an article on the generalization of quantum mechanics by biquaternions (*i.e.* on the Biquaternion Quantum Mechanics) in the journal, *Hadronic Journal*, that is directed by Santilli.

Sincerely, Elio Conte

****

Device Update continued from page 35

a comparison with pure joule input power vs. condensate. On a few attempts using it, we found that, like the electrolysis cell, it has top lid leaks that need fixing.

The condenser, a copper pipe jacketed by a larger PVC pipe in which cold water circulates, works well. Very little condensate collects on the inside walls of the condensate collection jar. Initial checks were made for pH of the condensate to be sure that it did not contain electrolyte by determining that it measured pH=7.0. We were excited to find some extraordinary quantities of what were initially assumed to be pure water condensate on some high power runs. Re-running the test determined that its pH 9.2 was, quite contaminated with electrolyte. The solution had blown past a long portion of uphill tubing with plastic mesh that is designed to "de-mist," then through a longer uphill piece of tube to enter the top of the condenser. The jury is still out on the cell's energy balance.

Hydrosonic Pump™ Preparations

The Griggs machine sits quietly in the corner, now wired with two thermocouples each on input and output and plumbed for operation. We are waiting on final details of approval from the power company before we can operate it.

Dennis Cravens-Dennis Letts Cell

The assortment of devices under test here at NERL is increas-

Device Update continues on page 63

New Energy Times Archive

A Chronology of Cold Fusion

Cold fusion did not arrive "out of the blue." It is part of the history of science, physics, chemistry, technology, and the history of hot fusion. Here is an eclectic view of the place of cold fusion in the larger history of science. Highlights of the years 1989 through the present are provided in more detail:

Circa 450 B.C.

• Greek philosophers Leucippus of Miletus and his student Democritus declare that the world is composed of indivisible particles — atoms.

500 B.C. - 1800 A.D.

• The age of alchemy, attempts to transmute base metals into gold or into an elixir for immortality. (Some forms of cold fusion circa 1989 appear to involve transmutations at low energy conditions, in reactions previously thought to be impossible.) "Modern Alchemy" is still alive and well throughout the 19th and 20th centuries.

1803

• British chemist John Dalton puts forth his *atomic theory*, which suggests that each element is composed of identical atoms whose properties determine the chemical reactivity of that element.

1816

• British physician William Prout suggests that all atoms are composed of hydrogen atoms.

1854

• German physicist Hermann Ludwig Ferdinand von Helmholtz suggests that the gravitational contraction of the Sun may explain its release of energy and emission of radiation.

1868

• Discovery of the element helium in the light spectrum of the Sun by Pierre Janssen and Joseph Norman Lockyer.

1869

• Russian chemist Dmitri Ivanovich Mendeleev proposes the periodic table of the elements, organizing the chemical elements according to their chemical properties.

1895

• Sir William Ramsay isolates the element helium in a uranium-bearing mineral.

• Serendipitous discovery of x-rays by Wilhelm Conrad Roentgen. Lord Kelvin's initial reaction is that the reports are a hoax.

1896

• The discovery of radioactivity by French physicist, Henri Becquerel.

1897

• Physicist Joseph John Thomson establishes the existence of electrons as extremely low mass carriers of negative electric charge. He suggests that electrons are embedded like raisins in the positively charged body of an atom.

1904

• Physicist Ernest Rutherford suggests that gravitational energy is inadequate to explain the energy release by the Sun.

1905

• Einstein proposes his theory of Special Relativity in which mass and energy are shown to be equivalent. The theory includes the famous relation $E=mc^2$.

1906-1908

• By bombarding materials with alpha particles from radioactive elements, physicist Ernest Rutherford in England develops the concept that an atom has a tiny, dense nucleus.

1909

• Rutherford identifies alpha particles as the nucleus of the helium atom.

1911

• Dutch physicist Heike Kammerlingh Onnes discovers the phenomenon of superconductivity in the element mercury chilled to 4.12 K. Superconductivity would later play a role in developing magnetic confinement hot fusion, and "high temperature superconductivity" would emerge only two years before the Utah cold fusion announcement in 1989.

1915

• American chemist William Draper Harkins suggests the possibility of fusing hydrogen atoms to produce helium, with a fraction of a percent of the original mass being converted to energy.

1919

• Rutherford transmutes nitrogen to oxygen in his alpha particle bombardment experiments—the first (known, confirmed) transmutation of elements accomplished by human beings. (Later confirmed by British physicist Patrick M.S. Blackett.)

1920

• Rutherford identifies the hydrogen nucleus as a fundamental particle, the proton.

• British astronomer Arthur Eddington demonstrates that the conversion of mass to energy could power the stars.

Early 1920s

• Irving Langmuir at General Electric Research Laboratory coins the term *plasma* for a partially ionized gas.

1926

• At the Chemical Institute of the University of Berlin, chemists Fritz Paneth and Kurt Peters investigate the possibility of transforming hydrogen to helium using a catalyst such as palladium. They claim to produce small amounts of helium by diffusing hydrogen through a capillary tube made of palladium. Report their results in two German scientific journals.

1927

• Building on the ideas of Paneth and Peters, chemist and physicist John Tandberg files a patent application in Sweden to use an electrochemical cell with a palladium electrode operating at high pressure to produce helium.

• After tests performed at Cornell University and in Berlin, Paneth and Peters formally retract their 1926 claims in two German journals as well as in *Nature*.

• John Tandberg's patent application is rejected because "the description is not complete enough that by its use an expert is likely to be able to practice the invention."

1929

• British astronomer Robert Atkinson and Austrian physicist Fritz Houtermans publish a speculative article suggesting a way for protons to fuse to form helium in a star via an intermediary "proton trap" nucleus.

1932

• Discovery of the neutron by British physicist James Chadwick.

• Chemist Harold Urey isolates the isotope of hydrogen, deuterium.

• John Tandberg in Sweden attempts to create nuclear fusion by electrolyzing heavy water with high energy electrical discharges in a palladium wire.

1934

• Australian physicist Marcus Oliphant and Austrian chemist Paul Harteck produce helium-3 by bombarding deuterium with deuterons.

1936

• Niels Bohr develops the liquid drop model of the nucleus.

• Physicist Carl Anderson at Caltech discovers the *mu meson* or *muon*., a particle that others would later use in *muon-catalyzed* cold fusion.

1938

• Otto Hahn and Fritz Strassman in Germany bombard uranium with neutrons and create the first man-made fission of an element. Lise Meitner, who had been working with Hahn before fleeing to Stockholm from Germany, identifies the process as fission.

1939

• Hans Bethe, Geroge Gamow, and Ralph Alpher publish detailed mechanism of fusion reactions powering the Sun.

1941

December 6

• President Roosevelt initiates the Manhattan nuclear bomb project.

1942

December 2 — 3:45 p.m.

• First self-sustaining fission reaction occurs in graphite pile built at the University of Chicago, under the direction of physicist Enrico Fermi.

1945

July 16 — 5:30 a.m.

• Detonation of world's first nuclear bomb at Alamogordo, New Mexico.

1945-1946

• At Los Alamos, physicists Edward Teller, Robert R. Wilson, and Robert Cornog discuss magnetic confinement schemes for plasmas to create controlled fusion. James L. Tuck and Stanislaw Ulam collaborate on the idea of colliding high velocity beams of deuterium.

New Energy Times Archive

1946

• Sir George P. Thomson and Moses Blackman in England file a patent application for a toroidal fusion reactor using the *pinch effect*.

1949

• Explosion of first Soviet fission bomb triggers U.S. effort to develop the hydrogen bomb.

Late 1940s

• Physicists Andrei Sakharov in the Soviet Union and F.C. Frank in England independently suggest the possibility of muon-catalyzed cold fusion.

1951

• Argentine dictator Juan Perón makes worldwide headlines by announcing that a German scientist working for him has invented controlled fusion. Perón suggests that within months Argentina will have a full-scale fusion power plant.

• Princeton University astronomer Lyman Spitzer, Jr., inspired by the Perón announcement, conceives the basic scheme of magnetic confinement of a plasma to produce controlled fusion.

1952

November 1

• Detonation of the world's first thermonuclear bomb by U.S. on the coral Pacific atoll, Elugelab.

1956

• Louis Alvarez and his colleagues at UC Berkeley accidentally discover muon-catalyzed cold fusion.

1958

• Harwell Weapons Laboratory in England announces success with a hot fusion experiment called "Zeta." Neutron measurements are found subsequently to be erroneous. "Science by press conference" —a first for hot fusion.

1965-1968

• Soviet scientists develop and announce their tokamak hot fusion reactor concept. Reaction in the West is extremely skeptical, yet it later becomes hot fusion dogma.

1969

• British measurements at a Soviet tokamak convince western scientists that the tokamak concept is really a breakthrough in magnetic fusion.

Late 1960s—Early 1970s

IE archives

• Martin Fleischmann and his colleagues conduct research on the separation of hydrogen and deuterium isotopes, work that leads in the 1980s to cold fusion experiments.

Mid-to late 1970s

• Beginnings of inertial confinement (laser) fusion efforts.

1978

•Russian physicist B.A. Mamyrin observes anomalies in the concentrations of helium-3 and helium-4 in metals, work that later influences Steve Jones and his colleagues.

August

• Tadahiko Mizuno and Kurachi in Japan observe anomalous 200 ml evaporation event in palladium-heavy-water electrolytic cell. Dismissed as "unsolvable mystery."

IE archives

Late 1970s

• B. Stanley Pons examines isotopic separation in palladium electrodes and is puzzled by some of his results.

1980

• U.S. Magnetic Fusion Energy Engineering Act of 1980 recommends a doubling of magnetic fusion budget within seven years.

1981

May

• Tadahiko Mizuno and Mori in Japan discover x-rays from an electrolytic cell when loading titanium with deuterium.

1983

• Alcator-C tokamak at MIT is the first tokamak to exceed the minimum Lawson parameter for breakeven (if fuel were deuterium-tritium).

Courtesy University of Utah

1984

• Pons and Fleischmann begin their collaboration on an electrochemical experiment to search for evidence of cold fusion.

1985

• Geneva summit between Reagan and Gorbachev launches the ITER program to build an "International Thermonuclear Experimental Reactor" involving the US, USSR, Japan, and the European community. (U.S. ITER involvement ends in 1998.)

1986

April-May

Courtesy Brigham Young Univ.

• Stephen E. Jones of Brigham Young University and his colleagues begin planning cold fusion experiments with metals.

• Jones and Clint van Siclen publish paper on "piezonuclear fusion."

1987

• Johann Rafelski and Steven E. Jones publish an article in the July 1987 issue of *Scientific American* titled "Cold Nuclear Fusion." They describe the process of muon- catalyzed fusion reactions.

1988

• TFTR tokamak at Princeton attains a plasma temperature of 300 million degrees K. Both JET tokamak in England and TFTR closely approach breakeven conditions.

1989

February 23

• Pons and Fleischmann visit Jones' group at Brigham Young University. Jones mentions intent to publish an article on his research and to deliver his paper to the American Physical Society Meeting on May 4, 1989. Pons and Fleischmann want to spend more time—perhaps 18 months longer—on their experiments before going public.

March 11

• Pons and Fleischmann submit their paper on cold fusion to the *Journal of Electroanalytical Chemistry and Interfacial Electrochemistry*.

March 23

• Martin Fleischmann and B. Stanley Pons announce at a press conference at the University of Utah in Salt Lake City that a "simple experiment results in sustained fusion at room temperature for the first time." The story is first reported by the *Financial Times* in England and the Wall *Street Journal* in the U.S., triggering a media avalanche of coverage that lasts for months. Scientists around the world immediately begin efforts to confirm or reject the Utah claims.

• Stephen E. Jones submits his paper to *Nature*.

March 24

• Governor Norm Bangerter of Utah announces the convening of a special session of the state legislature to approve $5 million to start an institute for cold fusion research.

March - April

• Major cold fusion experimental efforts begin in Japan and India.

April 7

• Utah legislature passes cold fusion research legislation.

April 8

• The University of Utah legislature meets to consider $5 million appropriation for cold fusion research and development. Money is set aside, not to be used unless the University of Utah experiments are confirmed.

April 10

• Researchers at Texas A&M University claim to have duplicated the Pons and Fleischmann experiment (this one group's claim later is found to be a mistake).

• Researchers at Georgia Tech announce the detection of neutrons from an electrochemical cell. (A claim subsequently retracted, three days later, due to mistake.)

• Pons and Fleischmann paper on cold fusion appears in the *Journal of Electroanalytical Chemistry and Interfacial Electrochemistry*.

April 12

• Meeting of the American Chemical Society in Dallas, Texas, is attended by some 7,000 chemists; Pons defends the alleged fusion breakthrough.

• Peter L. Hagelstein at MIT announces his explanation of cold fusion as "coherent fusion." MIT announces that it has applied for patents related to Hagelstein's theories.

• Professor Keith Johnson of MIT's Department of Materials Science and Engineering announces his theory to explain the University of Utah results on the basis of "dynamical Jahn-Teller" chemical bonding, while acknowledging that small amounts of nuclear fusion could be taking place in the palladium electrode. He is first to suggest publicly that heat may come from light water tests too.

April 14

• University of Utah chemistry professors Cheves T. Walling and John T. Simons announce their theory to explain the Pons-Fleischmann experiments.

Courtesy Lawrence Berkeley Laboratory

• Georgia Tech researchers retract their claimed neutron detection.

• Nobel Laureate Glenn T. Seaborg tells President

New Energy Times Archive

George Bush in the White House oval office that the Utah claims are "not fusion" but should be investigated anyway.

April 17

• At a University of Utah press conference, Pons announces that results of mass spectroscopy reveals quantities of helium-4 are being produced by their electrochemical cells.

April 18

• Italian physicist Francesco Scaramuzzi, with a group at the Italian Atomic Energy Authority at Frascati claims success in detecting neutrons from cold fusion using a titanium metal infused under pressure with deuterium.

•Robert Huggins at Stanford University, says he has obtained excess heat in an electrochemical cell with heavy water, but not in one with light water.

April 24

• Secretary of Energy Admiral James Watkins establishes a review panel to evaluate cold fusion claims. Directs DOE's National Laboratories to conduct cold fusion research investigations

April 26

• Members of the U.S. House of Representatives Committee on Science, Space, and Technology hear request from the University of Utah for quick approval of $25 million to help commercialize their alleged fusion process.

April 27

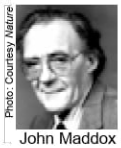


Photo: Courtesy *Nature*
John Maddox

• A paper by Steven E. Jones and seven colleagues appears in *Nature*, titled "Observation of Cold Nuclear Fusion in Condensed Matter." Editorial by John Maddox in *Nature* is skeptical of Fleischmann and Pons cold fusion claims.

April 28

• Yale University-Brookhaven National Laboratory collaboration reports detecting no nuclear products from electrochemical cells after three weeks of trying.

• Researchers at Case Western Reserve University report excess heat and tritium generation in electrochemical cells.

May 1

• *Boston Herald* publishes attack on Fleischmann and Pons by MIT Plasma Fusion Center Director Ronald R. Parker

May 1 and 2

• Researchers at MIT Plasma Fusion Center say they have found serious flaws in the neutron emissions data reported by Pons and Fleischmann; say that Pons and Fleischmann were unlikely to have detected neutrons. At a meeting of the American Physical Society in Baltimore, Maryland, physicists and chemists roundly criticize and mock the work of Pons and Fleischmann.

Photo by Robert Paz, 1984
Prof. Steve Koonin of CalTech, one of the most outspoken critics.

May 8

• At meeting of the Electrochemical Society in Los Angeles, Pons and Fleischmann defend their experiments against criticism, but back off on their claims to have detected neutrons.

May 18

• An extended scientific correspondence by researchers at MIT's Plasma Fusion Center appears in *Nature*, critiquing the original Pons and Fleischmann neutron data and finding it to be flawed.

May 23-25

• The first conference dedicated exclusively to cold fusion is held in Santa Fe, New Mexico, and draws some 500 participants.

June 15

• Harwell Laboratory in England stops work on cold fusion experiments.

June 26

• Los Alamos National Laboratory scientists announce finding tritium in cold fusion experiments.

• MIT Plasma Fusion Center holds "Wake for Cold Fusion" party.

Late June

• Release of funding by Utah State Fusion Energy Advisory Council to establish the National Cold Fusion Institute.

July 12

• Interim report by ERAB panel of DoE finds no good evidence for cold fusion and rejects a formal research center for cold fusion.

July 22

•International Electrochemical Society Meeting. Papers presented by Fleischmann, Pons, and Bockris.

July 31

• First formal conference on cold fusion in Japan, under the auspices of the International Electrochemical Society.

October 16-18

• EPRI/NSF meeting on cold fusion in Washington, D.C.

October 19-20

•Electrochemical Society meeting, Hollywood, Florida, cold fusion session.

November

•Astronomer Carl Sagan in Ithaca, New York warns science journalists at their annual CASW meeting to keep an open mind about cold fusion and not to be guided by "polling." His words go unheeded.

November 8

• Final DoE ERAB report approved concluding that evidence for cold fusion is not persuasive.

Dr. William Happer
DoE ERAB panel member
Photo: Princeton Univ.

Dr. John Huizenga
DoE ERAB panel co-chairman
Photo: Univ. of Rochester

1990

March 28 -31

• First Annual Conference on Cold Fusion held in Salt Lake City is attended by over 200 researchers.

•*Nature* magazine (March 29) publishes scathing editorial attacks on cold fusion research and prints an article by University of Utah physicist Michael Salamon *et al.* claiming that in 1989 tests they detected no nuclear products coming from Pons's cells.

June 15

• Publication in *Science* of a lengthy exposé by Gary Taubes in which he strongly suggests that high levels of tritium found in Texas A&M University cold fusion experiments were the result of fraudulent spiking of the cells.

July 23

• Bruce Liebert, Bor Yann Liaw and colleagues at the University of Hawaii report multi-watt excess power output from electrochemical cells incorporating molten salts and deuterium. Results reported at the cold fusion sessions of the World Hydrogen Energy Conference.

July 25

• A comprehensive paper on excess-heat measurements in electrochemical cells is published by Fleischmann, Pons, *et al.* in the *Journal of Electroanalytical Chemistry*.

October 22-24

• Conference on "Anomalous Nuclear Effects in Deuterium/Solid Systems" at Brigham Young University. Major confirming evidence presented that finds energetic tritons in cold fusion experiments. Conference produces clinching proof that cold fusion can occur in deuterated systems without being catalyzed by muons.

November 7

• Review meeting in Salt Lake City of the Fusion Energy Advisory Council. Stanley Pons attends after returning from an alleged "disappearance" in Europe.

Mid-December

• Review panel gives NCFI operations generally favorable critique.

1991

March 17

• Sunday *New York Times*, front page: Frank Close book, *Too Hot to Handle* profiled— accusations of ethical violations against Pons and Fleischmann.

March 22-26

• First Soviet National Conference on Cold Fusion, Dubna-Moscow. Research at 45 institutes reported, eighty papers submitted.

April 6

• Tadahiko Mizuno observes astonishing "heat-after-death" incident in cold fusion cell with power turned off. (See detailed account in *Nuclear Transmutation: The Reality of Cold Fusion*, Infinite Energy Press, 1998.)

April 24

•Mayer-Reitz "hydron" theory of cold fusion announced.

• Randell Mills announces "shrinking hydrogen atom" (hydrino) theory and experimental evidence for significant excess heat in light-water, nickel, potassium carbonate cells.

Late May

• Eugene Mallove's cold fusion book, *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor,* is published by John Wiley & Sons. It calls evidence for cold fusion "overwhelmingly compelling."

June 7

• Frank Close gives invited talk, "Exposé on Cold Fusion," at stormy session at MIT Plasma Fusion Center in which he attacks Pons and Fleischmann's work.

•Eugene Mallove formally resigns his position as Chief Science Writer at the MIT News Office, citing ethical violations by MIT researchers who attacked cold fusion.

New Energy Times Archive

June 29-July 4

• Second Annual Conference on Cold Fusion, Como, Italy.

June

• Tadahiko Mizuno finds black deposit on cold fusion cathode, scrapes it away, believing it to be contamination. It may have been visible evidence of transmutation.

June 30

• National Cold Fusion Institute closes.

August 18

•Eugene Mallove files formal request for investigation of mis-handling of data in 1989 MIT PFC cold fusion calorimetry experiment.

December 18

• Martin Fleischmann speaks at MIT on cold fusion research.

1992

January 2

•Andrew Riley killed in explosion at SRI International during cold fusion experiment.

January 12

• Eugene Mallove in Concord, New Hampshire questions presidential candidate William Jefferson Clinton about cold fusion. Clinton admits knowing that scientists in Arkansas were "stonewalled" by the U.S. Department of Energy. Promises to do something about investigating cold fusion (but never does after being elected President).

January 27

•ISEM, IEEE meeting of cold fusion researchers, Nagoya, Japan. Akito Takahashi of Osaka University reports massive excess heat and low-level neutrons.

March

•Publication by Drs. Noninski of positive results in excess heat measurement in nickel, light water, potassium carbonate system.

April 1

• Eugene Mallove testifies favoring cold fusion research before Subcommittee on Energy and Water of the U.S. House Appropriations Committee.

• Final letter from President Vest closing MIT PFC calorimetry matter, without final resolution.

April 15

•Professor Akito Takahashi gives seminar at MIT on his heat and neutron-producing cold fusion experiments.

May

•MIT PFC publishes single-author "Technical Appendix" to sixteen-author 1989 cold fusion calorimetry experiment report, expands error limits.

July 10

• Japan's MITI announces major funding of cold fusion research.

Mid-July

• Publication of John Huizenga's *Cold Fusion: Scientific Fiasco of the Century*.

July 27

•Edmund Storms of Los Alamos National Laboratory announces replication of Takahashi excess heat.

August

•Mitchell R. Swartz, an MIT graduate, publishes "Reexamination of a Key Cold Fusion Experiment: 'Phase-II' Calorimetry by the MIT Plasma Fusion Center" in *Fusion Facts*. Analysis finds numerous faults in the analysis, as well as proof that data were inappropriately handled.

September 24

•McKubre of SRI International speaks about excess heat results at MIT.

Late September

•Electrochemical Society Meeting in Japan; many excess heat reports.

October 21-25

• Nagoya Third International Conference on Cold Fusion. Numerous results confirm reproducibility of excess heat and nuclear phenomena.

November 17

• NTT Laboratories announces $565,000 "Cold Fusion Kit."

December 3-4, 9-10

• Dr. Reiko Notoya of Hokkaido University visits MIT to demonstrate her light water cold fusion experiment.

1993

March 25

•Congressman Dick Swett (New Hampshire) and Eugene Mallove testify before the House Appropriations Committee, Subcommittee on Energy and Water to ask for funding for a National Academy of Sciences review of cold fusion.

March 29, 1993

•Arthur C. Clarke delivers speech, "2001: The Coming Age of Hydrogen Power," in Colombo, Sri Lanka. An address on cold fusion for the Pacific Area Senior Officer Logistics Seminar (PASOLS).

IE archives

May 5

•Fusion Energy Hearings before House Subcommittee on Energy of House Science, Space, and Technology Committee. Cold fusion researchers receive warm reception.

June

•Publication of Gary Taubes' *Bad Science: The Short Life and Weird Times of Cold Fusion*.

June 24

• World premier of Canadian Broadcasting Corporation's video documentary "The Secret Life of Cold Fusion."

August

•*Popular Science* publishes high-profile, objective account of latest cold fusion developments—a cover story by *Wall Street Journal* reporter Jerry Bishop.

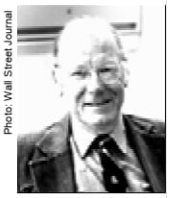


Photo: Wall Street Journal

Jerry Bishop

September

•HydroSonic™ Pump technology emerges as a significant probable excess-energy producing device. The horizons of the cold fusion field expand dramatically.

December 6-9

• Fourth International Conference on Cold Fusion, Maui, Hawaii, sponsored by the Electric Power Research Institute

• *"Cold Fusion"* Magazine is launched, initiated by Eugene Mallove, Jed Rothwell, Chris Tinsley, Larry Forsley, and others in the cold fusion field. Three issues are published. (All editors leave WGI publishing firm in June 1994 because of chaos in WGI management. Former editors begin preparations for new publication, *Infinite Energy*.)

1994

February

•Piantelli-Focardi-Habel announce new ordinary hydrogen-nickel high temperature excess heat results.

March 21 and March 26

•Landmark CBC (Canadian Broadcasting Corporation) and BBC (British broadcasting Corporation) TV programs on cold fusion: "Too Close to the Sun"

May 24-26

•International Symposium on Cold Fusion and Advanced Energy Sources, Minsk, Belarus.

May/June

•MIT *Technology Review* cover story, "Warming Up to Cold Fusion," by Dr. Edmund Storms. Angry letters follow.

June

•Professor Keith Johnson of the MIT Department of Materials Science announces that his techno-thriller movie script, "Excess Heat" (later called "Breaking Symmetry") will be produced.

July 16

•Nobel Laureate and leading cold fusion theorist Julian Schwinger dies after the sudden onset of an illness.

October

•E-Quest Sciences announces large amounts of helium production in test of ultrasonic cold fusion reactor at Los Alamos National Laboratory

•Y. Arata and Y.C. Zhang in Japan publish a new "double cathode" method of reliably producing the Pons-Fleischmann excess heat effect.

1995

January 21

• Cold Fusion Day at MIT—an IAP Program—Sponsored by Cold Fusion Technology.

March 24-25

•Workshop on the Status of Cold Fusion in Siena, Italy.

April

•*Infinite Energy* Magazine launched with Issue #1 (designated March-April 1995).

April 3

•Cold Fusion Poster session at American Chemical Society Meeting, Anaheim, California.

April 9-13

•Fifth International Conference on Cold Fusion (ICCF-5), Monte Carlo, Monaco.

May 2

•MIT receives significant U.S. patent (#5,411,654) relating to cold fusion, "Method of Maximizing Anharmonic Oscillations in Deuterated Alloys," by Brian S. Ahern, Keith H. Johnson, and Harry R. Clark, Jr.

June 19

•First Low-Energy Transmutation Meeting at Texas A&M University.

August 3

•European Patent Office approves patent by Italian group of F. Piantelli *et al.*, "Energy Generation and Generator by Means of Anharmonic Stimulated Fusion." (Patent No. WO 95/20816)

August

•*A Dialogue on Chemically Induced Nuclear Effects* by Nate Hoffman published by the American Nuclear Society.

September

•European Patent Office gives notice that it will grant the Pons-Fleischmann Patent "Method

New Energy Times Archive

and Apparatus for Power Generation."

October 1-5

•16th Biannual Symposium on Fusion Energy, SOFE '95 (Champaign, IL), CETI demonstrates Patterson Power Cell at 80:1 power ratio.

October 1-8

•Third Russian Conference on Cold Fusion, Sochi, Russia.

December 5-7

•CETI demonstrates Patterson Power Cell at Power Gen '95 in Anaheim, CA: 1,300 watts out, 1.4 watts in.

1996

January 20

•Cold Fusion and New Energy Symposium Sponsored by *Infinite Energy* Magazine at Cambridge Marriott Hotel.

February 7

•ABC-TV programs: "Good Morning America" and "NightLine" feature cold fusion and the Patterson Power Cell.

February

•NASA Lewis Research Center publishes report, TM 107167 "Replication of the Apparent Excess Heat Effect in a Light Water, Potassium Carbonate, Nickel Electrolytic Cell."

February 22-23

•1996 New Hydrogen Energy Symposium, Sapporo, Japan.

March 2

•"Normal Temperature Nuclear Fusion" (cold fusion) conference in China.

May 19-26

•Fourth Russian Conference on Cold Fusion and Nuclear Transmutation, Sochi, Russia.

September 13-14

•2nd Low Energy Transmutation Meeting, College Station, Texas. Permission refused to hold conference on campus of Texas A&M as "cold fusion is impossible." Conference moved to a hotel.

October 13-18

• Sixth International Conference on Cold Fusion, Hokkaido, Japan.

• *Infinite Energy* publishes classic paper by Prof. George Miley and James Patterson, "Nuclear Transmutations in Thin-Film Nickel Coatings Undergoing Electrolysis."

November 11-2

•At American Nuclear Society Meeting in Washington, D.C., Clean Energy Technologies, Inc. (CETI) announces leasing program for new hydrogen demonstration cells that exhibit low-energy nuclear transmutations.

1997

February 21

•Just as it was about to be given final aproval, the Pons and Fleischmann cold fusion patent was effectively killed at the European Patent Office after CETI opposed its being granted.

April 4

•Paramount Pictures' movie, *The Saint*, opens in theaters across the U.S. The movie features a cold fusion theme and stars Val Kilmer and Elisabeth Shue.

May 23-26

•International Symposium on New Energy, Denver, Colorado.

June

• The "Cincinnati Group" announces its process of radioactive waste remediation and offers a "LENT-1" thorium activity reduction demonstration cell as a commercial product. Detailed results published in *Infinite Energy*, Issue No. 13/14.

June 1-5

•American Nuclear Society Annual Meeting in Orlando, Florida features "Low Energy Nuclear Reactions Panel Session."

June 10

• Office of Energy Intelligence at U.S. Department of Energy sends supportive letter to cold fusion company, Clean Energy Technologies, Inc. Praises "the development of what looks to be a most promising innovation in energy production."

June 11

•ABC TV "Good Morning America" and "NightLine" feature CETI's Patterson Power Cell on the same day.

June

•ENECO of Salt Lake City is forced to abandon its attempt to get the Pons-Fleischmann cold fusion patent approved by the USPTO.

July

•IECEC-32, Hawaii. Session on transmutation in cold fusion experiments.

• July/August issue of *Analog* magazine features "Cold Fusion: The Miracle Was No Mistake" by Eugene Mallove.

September

•*TWA Ambassador* in-flight magazine publishes cover story about cold fusion controversy.

•September 11

•Cold fusion pioneer Kevin Wolf of Texas A&M University dies at age 55.

September 29-October 5

•Fifth Russian Conference on Cold Nuclear Transmutation (RCCNT-5), Sochi, Russia.

September 30

• U.S. Patent 5,672,259 issued to James Patterson, "System with Electrolytic Cell and Method for Producing Heat and Reducing Radioactivity of a Radioactive Material by Electrolysis."

October 9

• 1997 "Ig-Nobel" Prize in Physics awarded to Prof. John O' M. Bockris "for his wide-ranging experiments in cold fusion. . .etc."

November 27-30

•Cold Fusion Conference held in Asti, Italy, "Asti Workshop on Anomalies in Hydrogen/Deuterium Loaded Metals."

December

•Tadahiko Mizuno's book, *Nuclear Transmutation: The Reality of Cold Fusion*, published in Japanese by Kogakushu. First print run sells out.

• New Energy Partners, cold fusion and new energy capital fund (Greenich, Connecticut), begins investment in the cold fusion field.

1998

April

•Collapse of the Japan "New Hydrogen Energy" program due to failed management.

April 19-24

•Seventh International Conference on Cold Fusion (ICCF-7) meets in Vancouver, British Columbia, Canada.

• Chemical engineer Dr. Les Case announces "catalytic fusion" process that creates excess heat and helium-4 with special off-the-shelf catalysts.

May 4

•Professor Makoto Okamoto, a leading cold fusion researcher, dies at age 60.

June 5

• In provocative *Science* magazine essay, Arthur C. Clarke supports openness to cold fusion. Calls ignoring of cold fusion "one of the greatest scandals in the history of science."

June 8-11

• Special sessions and Workshop on Low Energy Nuclear Reactions at the American Nuclear Society (ANS) Annual Meeting, Nashville, Tennessee.

September 28-October 5

•Sixth Russian Conference on Cold Fusion, Dagomys, Russia.

October

U.S. Participation in ITER Hot Fusion Program ended by Congress.

October 11

•Cold Fusion and New Energy Symposium, Manchester, New Hampshire (sponsored by *Infinite Energy* Magazine).

November

•*Wired* Magazine publishes Charles Platt's feature story on cold fusion, "Dirty Science: The Strange Rebirth of Cold Fusion"—featured on the cover.

December

•Tadahiko Mizuno's book, *Nuclear Transmutation: The Reality of Cold Fusion*, published in English by Infinite Energy Press, Concord, New Hampshire.

1999

March

• *Infinite Energy* publishes Special Tenth Anniversary Issue which includes an MIT Special Report.

1999/2000

April

Video Documentary Debuts: *Cold Fusion: Fire From Water*

April 29 - May 1

Conference on Free Energy at U.S. State Department. Includes Cold Fusion.

• The Eight International Conference on Cold Fusion (ICCF-8) meets in Italy at a nuclear facility.

"Leave the world a better place than you found it." —Chris Tinsley

New Energy Times Archive

Rothwell continued from page 24

going to succeed anywhere, it will have to be in the marketplace, on Wall Street, on television talk shows, in mass media articles like the recent *Wired* article, and as Scott Chubb suggests, in "the Internet, schools, and even churches and synagogues." Ultimately this battle must be fought in the halls of Congress, because this is about politics, not science. The scientific issues were settled when NCFI, Los Alamos and SRI published in 1990.

The prospect of an ugly public confrontation might upset academic scientists, but I find it refreshing. I have more faith in the common sense of ordinary people than in the vaunted opinions of elite decision-makers. Our society works best when ordinary people lead, and the elites follow.

I think Jaeger has lost sight of the fact that we already have extraordinary results. We should be optimistic! The NCFI saw tritium in four out of four experiments. Claytor *et al.* at Los Alamos have also achieved highly reproducible tritium production. McKubre observed 200% excess heat at Sigma 90, and he declared that, "The effect is thus neither small nor fleeting." Storms published recipes for skilled electrochemists showing how to achieve reproducibility of 50% or better. Bockris, Mizuno and Miley have observed host metal transmutations. In six out of six experiments, Iwamura at the Mitsubishi Advanced Technical Research Center detected excess heat, x-rays and transmutations. These marvelous results were achieved working on a shoestring budget. Imagine how much more we could achieve with proper funding.

Perhaps we take old results for granted, as if the years since they were done and the multiple replications that followed somehow diminished the glory, and made these astonishing discoveries mundane. Years ago I spoke to a scientist who had done a cold fusion experiment with what he called mixed success. He said he detected low-level neutrons and tritium, but nowhere near as many neutrons as you would expect from a hot fusion reaction. I asked whether he saw enough neutrons and tritium to be sure they were real. He said yes, the levels were high enough above the noise for confidence, but not as high as he hoped, or as high as conventional plasma fusion theory predicts. I suggested a parable: A traveler is told that he should climb a certain high, mountain where he will meet a heavenly host of angels singing in harmony. He climbs the mountain. Weeks later he returns to the village looking glum. He says he met only two angels and they did not sing. A little tritium and a few neutrons—no matter how difficult they may be to replicate—are unexplained miracles. As EPRI put it, "Small but definite evidence of nuclear reactions have been detected at levels some forty orders of magnitude greater than predicted by conventional nuclear theory."[5] We cannot ignore this definite evidence just because the ratio of neutrons to tritium and heat seems peculiar. That is as true today as it was before 1989, when no one would have predicted any level of tritium from any palladium electrolysis experiment.

Cold fusion scientists have developed a siege mentality. They feel isolated. They do not realize that the public is sympathetic. We have found people are enthusiastic, and anxious to hear more. Scientists should open up to the world, lecture, write in the newspaper, appear on local cable TV talk shows, and invite reporters and investors in for a look.

Some scientists complain they cannot get funding for cold fusion projects. Yet, when we have offered to introduce them to venture capitalists, they refused to set up demonstrations or spell out cell performance specifications. A working cold fusion cell would instantly attract millions of dollars, if it was presented properly, even if it was still far from being a practical source of energy. We are living through the largest speculative boom in history. Billions of dollars are being poured into riskier ventures than cold fusion.

Cold fusion scientists would achieve a great deal simply by telling people what they have accomplished. We should examine our own behavior not because we are the main culprits (although there is plenty of blame to go around), but because we can change our own behavior, and alter our own tactics; but we have no control over the opposition. Technical progress and good experiments alone will not convince the public and public support is essential.

References

1. F. G. Will, K. Cedzynska, M-C. Yang, J. R. Peterson, H.E. Bergeson, S. C. Barrowes, W. J. West and D. C. Linton (National Cold Fusion Inst., University of Utah, USA), "Studies of Electrolytic and Gas Phase Loading of Palladium and Deuterium," presented at Second Annual Conference on Cold Fusion, June 29-July 4, 1991, Como, Italy.

2. E. Mallove, *Fire from Ice*, (John Wiley & Sons, May, 1991), p. 238.

3. See, for example, R. J. Hawryluk *et al.*, "Confinement and Heating of a Deuterium-Tritium Plasma," PPPL-2977, Princeton University, February 1994.

4. McKubre, M. C. H. *et al.*, Development of Advanced Concepts for Nuclear Processes in Deuterated Metals, EPRI TR-104195, Research Project 3170-01, August 1994

5. McKubre, *ibid.*

✧ ✦ ✦ ✧

Device Update continued from page 57

ing. We recently purchased a Pons/Fleischmann-type heavy-water, *closed* cell from Dennis Cravens that he and Dennis Letts have worked on together to investigate the high-frequency RF (82 MHz) electromagnetic stimulation of a Pd cathode. Dennis Letts reports recent success in boosting the DC excess energy of such a cell with 82 MHz RF of insignificant power. We are in the calibration and cathode loading stages of this investigation.

The flow-calorimetry system employs a stainless steel Dewar within a larger tempering beaker. The cell under test rests in the Dewar and is tightly wrapped with a copper coil. The Teflon cap of the cell seals the contents and has a recombiner built into it. Cooling water that runs through the copper coil also runs through the tempering beaker, a peristaltic pump, a pulse damping device, and an electronic flowmeter. A reservoir is provided, which allows for easy periodic checking of the flow rate using a graduated cylinder and a stop watch. As with most experimental devices, this one has many subtleties. Dennis Cravens warns us that we have to think very carefully about how heat travels when assembling the device, for it is packed in insulation that should not get moist. Moist insulation can act as a heat wick.

The tempering beaker serves to provide a stable environment for the Dewar. It has a thermostatically controlled heating pad wrapped around it to maintain a 30°C water temperature. This is a large, stable thermal mass for measurement stability.

Catalytic Fusion Cell of Dr. Les Case

Work has continued in attempting full-calorimetric measurement on a catalytic fusion cell of Dr. Case's design. We have obtained a new WW-II oxygen cylinder cell from him, but have encountered measurement problems with the present set up (see *IE* No. 23). We will be revising it. ✧ ✦ ✦ ✧

COLD FUSION TIMES

The Journal of the Scientific Aspects of Loading Isotopic Fuels Into Materials

Published Quarterly

Publisher and Editor, Dr. Mitchell Swartz

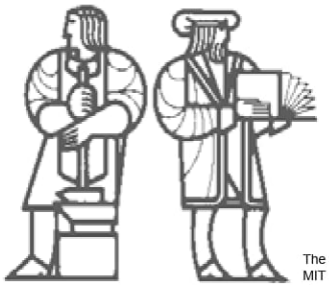
Annual subscription rate is $120.00. Single issues are $40.00. Outside U.S. extra $5.00 postage/handling

P.O. Box 81135

Wellesley Hills, MA 02181 USA

New Energy Times Archive

MENS ET MANUS [MIT Motto—"Mind and Hand"]



The Official MIT Logo

Why "MIT and Cold Fusion"?

by Eugene F. Mallove, Sc.D.

MIT Class of 1969 (Aero/Astro Engineering '69 SB; '70 SM)

Chief Science Writer, MIT News Office 1987-1991

MIT has played an extraordinary role in the history of cold fusion. By acts of commission and omission it continues to do so. On the occasion of the tenth anniversary of the startling announcement by Drs. Fleischmann and Pons on March 23, 1989, it is imperative that *Infinite Energy* explore the major role of MIT in shaping the history of the investigation of cold fusion.

Excess power evolution and unexpected ("impossible") nuclear changes in hydrogen-metal systems come under the rubric "cold fusion." Whatever its complete microphysical explanation turns out to be, cold fusion is of surpassing importance from the perspective of both science and technology. MIT's role in this affair bears close scrutiny by all who value what they assume MIT stands for: the open-minded quest for the truth about Nature and the application of new discoveries in science toward the betterment of humanity.

I defy any previously uninvolved MIT student or graduate to examine the thirty-four references catalogued on pages 29-34 of this issue ("Key Experiments that Substantiate Cold Fusion Phenomena") and conclude that the information is not strong enough to warrant further investigation and action.

This report will be of special interest to all who are concerned about the well-being of MIT—its alumni/ae, students, faculty, and administration. What this brief history says about the actions and inactions in the area of cold fusion by one of the world's great technical universities has far-reaching implications for *everyone* interested in the heated cold fusion controversy. The history of MIT's reaction to cold fusion will become a remarkable case study in how a major scientific revolution is affected by the strong news media influence of MIT, by government funding of MIT, and by the scientific involvement of MIT professors, administration, students, and staff.

Extraordinary circumstances demand extraordinary action. It is our obligation—our moral imperative—to publish the detailed report that follows. Unfortunately for the world, many people still believe that the claims of a new, clean, abundant energy source and nuclear reactions occurring near room temperature were quickly and definitively disposed of by the careful work of scientists at MIT in the spring of 1989. Nothing could be further from the truth. These investigators at MIT did *not* produce definitive work. In fact, quite the contrary. A great opportunity for pioneering by MIT was missed and the baby *was* thrown out with the bath water—at least temporarily.

The actions of certain MIT staff members in 1989 were a major influence on the news media, on other scientists, and on the funding support for cold fusion. This is a matter of record. Though a small group of open-minded, involved faculty, staff, and alumni pursued and continue to pursue cold fusion, MIT as a whole did, indeed, acquire the deserved reputation as a "Bastion of Skepticism" on cold fusion. Sad to say, it was initially only a handful of MIT staff and faculty who gave MIT this reputation. They inappropriately drove many others—on campus and off—to dismiss the claims from Utah in 1989 and the research that has followed. Thus, the role of MIT in cold fusion—apart from the stellar accomplishments of those who persevered in scientific investigations—must be regarded as a permanent blemish on MIT's otherwise undisputed role as a leader in science, technology, and education. Fortunately, it is a bad mark that could be expunged by future good deeds—and apologies for past *mis*deeds. Is the MIT of 1999 up to that? We shall see.

One hopes that this true characterization of MIT's institutional behavior in the early 1990s and beyond will be but a temporary aberration. Yet if the past is any guide, there is little cause for optimism that a sudden awakening to the truth will occur at MIT. Perhaps the greatest hope lies in the youth—the students and graduates of MIT who will examine the scientific literature objectively. Most MIT professors today are simply oblivious to the subject. If they were to examine the research record of the past decade, they would readily see the opportunities to enter what is clearly an area of enormous potential. MIT students and alumni/ae may need to become catalysts that move faculty members and administration in the right direction, away from the present untenable position of denying well-established experimental facts and the theoretical developments by Professors such as Peter L. Hagelstein (Electrical Engineering and Computer Science) and Keith H. Johnson, formerly of the Department of Materials Science and Engineering.

I defy any previously uninvolved MIT student or graduate to examine the thirty-four references that are cataloged on pages 29-34 of this issue ("Key Experiments that Substantiate Cold Fusion Phenomena") and conclude that the information is not strong enough to warrant further investigation and action. These are only a small sample of many other papers and developments that can be cited. In 1999, it is possible for MIT graduates to visit laboratories in the U.S. and abroad where cold fusion investigation and development are moving forward. And soon enough there will be a host of demonstration sets and kits that research laboratories can purchase to observe the effects themselves. Some of these will be distributed by companies in which MIT graduates are involved.

The events of 1989-1992 are past history, but one must learn from the past or be condemned to repeat it. I hope that MIT students will also study the wrongs that have been done by MIT faculty and staff, which perverted the process of science in this area. Ironically, those very faculty and staff who so loudly pontificated about the *alleged* unethical actions of cold fusion

New Energy Times Archive

researchers Drs. Martin Fleischmann and Stanley Pons are themselves most culpable. They launched distortions about cold fusion that have gained such wide currency.

As the record shows, the first assault against the truth in 1989 was press manipulation by faculty members engaged in the lavishly funded hot fusion research at MIT's Plasma Fusion Center (PFC). They did not believe the Utah work at all. They suspected that Pons and Fleischmann were engaged in a "scam," and they were concerned that if the public were to have a too open-minded attitude toward the prospect of cold fusion as an energy solution, funding for their beleaguered thermonuclear program would be endangered—even more so than in its perennial brushes with budgetary extinction.

The truth about the calorimetry experiment performed at MIT in 1989 under DoE contract funding (DoE Contract DE-ACO2-78ET51013) is stark and unambiguous. Its purported "negative" result was used to influence the U.S. Department of Energy's rushed 1989 report against cold fusion. In alphabetical sequence, it is the very first report cited in the U.S. DoE's ERAB (Energy Research Advisory Board) Cold Fusion Panel report of 1989. Some would characterize the data manipulation in the sixteen-author MIT paper of 1989 as mere "data fudging." We do not mince words: the use of improperly handled scientific data to draw in the public mind and in the mind of the scientific community a completely false conclusion about an emerging discovery of overarching importance to humankind is high-level scientific misconduct, plain and simple.

Boston Herald

Telephone (617) 426-3000 ★★★ Monday, May 1, 1989

MIT bombshell knocks fusion 'breakthrough' cold

We do not know for certain who unethically manipulated the data, and that is not important, but it was, indeed, inappropriately manipulated. "Inappropriately manipulated" is actually a very charitable way of describing what was done. We *do* know, however, that this erroneous study in the spring of 1989 at the MIT Plasma Fusion Center was *defended* by then Plasma Fusion Center Director Ronald R. Parker. Parker continues to play a leading role in hot fusion. For several years after leaving the MIT PFC, he was stationed at the ITER (International Thermonuclear Experimental Reactor) in Garching, Germany. Since 1989, the U.S. Government has funnelled billions of dollars into magnetically confined thermonuclear fusion development on projects, such as ITER. Though ITER funding was recently killed by the U.S. Congress, funding of tokamak hot fusion continues at MIT and elsewhere.

The record is clear: Had MIT researchers behaved responsibly and ethically as scientists in the spring of 1989, it is most probable that a position of open-mindedness by MIT on the difficult subject of verifying the Utah claims would have averted the highly negative U.S. Department of Energy Report drafted in the summer of 1989. History would have been far different. Most likely, expensive engineering programs aimed at hot fusion reactors would have been cancelled in the early 1990s; plasma physics studies would have continued at MIT, and MIT researchers, including those at the Plasma Fusion Center, might have become the most eminent cold fusion researchers in the world.

It was not to be. Cool heads could have reserved judgement. They could have followed the experimental facts where they would ultimately lead, but they chose not to. Heads were not cool, they were hot. MIT could have been in the vanguard of the new scientific field as befits its leadership role in science, but this did not happen. MIT chose—and is continuing to choose—defense of its existing professional support from the Federal government over meticulously documented evidence of a new scientific field and the pathway to revolutionary technologies. In fact, the current President of MIT, Dr. Charles M. Vest, who ignored my written concerns in 1991-1992, is on a Federal panel that now has major impact on U.S. DoE energy research funding (see pages 79). Dr. Vest played a key role in papering over the misdeeds of 1989, as the following report clearly shows.

To use press manipulation and data manipulation to misdirect billions of dollars in Federal scientific funding is scandal of the highest order. To coin a phrase in this era of various "-Gate" scandals and cover-ups—this is HeavyWatergate, one of the greatest (but still to be acknowledged) scandals in the history of science.

To use the phrase "scientific schlock" which then MIT Plasma Fusion Center Director, Prof. Ronald R. Parker used against Fleischmann and Pons' work in 1989 (and later falsely denied using it, perhaps fearing legal action), aptly characterizes the methods used by certain MIT researchers against the new science of low-energy nuclear reactions. It was not only data manipulation or "processing," as Parker later would contend on the 1994 BBC and Canadian Broadcasting Corporation cold fusion documentary, *Too Close to the Sun*, but there was a whole range of dirty tricks, deceptions, and self-deceptions that MIT professors and senior officials employed against cold fusion. We have catalogued most of them; this report is lengthy, but it is not comprehensive. Still, we urge you to read this history and learn from it. Perhaps concerned MIT graduates or faculty will have something to say about this. I encourage your feedback.

Some may say, "Why drag up the negative past, why not just emphasize the positive in the pages of this magazine?" To some degree that argument has merit, and we would like to be as positive as we can be. But we cannot ignore the reality that MIT's reputation as a "bastion of skepticism" against cold fusion has had a devastating effect on the progress of scientific investigation. As an article in *Infinite Energy*, Issue No. 11, revealed, the U.S. Patent and Trademark Office is using bogus conclusions from MIT investigators in 1989 to deny U.S. citizens their Constitutional rights to be granted patents on their intellectual property. Some of those—at least two to my knowledge—are MIT graduates! If generations of MIT students, alumni/ae and professors do not learn from the tragic errors of the past, how can the future be anything but dim?

How can the scientific and technological might and the vast resources of MIT be turned to this most important scientific problem of cold fusion and new energy, if the subject is relegated to nonsense and pseudoscience by past unacknowledged misdeeds—done by *individuals*, but with the implied imprint of MIT? How many scientists are not tuned into cold fusion pre-

Why MIT continues on page 118

New Energy Times Archive

MIT and Cold Fusion: A Special Report

Compiled and written by Eugene F. Mallove, Sc.D.

Photo: E. Mallove

Introduction

When on March 23, 1989 Drs. Martin Fleischmann and Stanley Pons announced that they had measured nuclear-scale excess energy from a palladium-heavy water electrochemical cell, and that they had also detected some preliminary evidence of nuclear signatures from their exotic energy-producing reactions, the world was in awe. Their famous afternoon press conference at the University of Utah, coming less than twelve hours before the Exxon Valdez ran aground off Alaska's pristine coast and spilled millions of gallons of oil, reminded us of the serious problems linked with fossil fuel dependency. The Chernobyl nuclear reactor accident of 1986 also hovered in the background. It was already clear that conventional fission nuclear power was in deep political trouble in many countries. The close coupling of energy and the environment was growing ever more apparent.

Following the Utah disclosure, the prospect loomed of a quantum leap in energy technology— a solution to the dilemma of fossil fuel domination and its threats to the environment and world peace. The Utah claims soon came to be known as "cold fusion," because the electrochemists were saying that they had solved the problem that the plasma physicists and engineers in the "hot" fusion program had been working on for four decades.

The hot fusioneers had been trying to mimic the stars—to "bring the power of the Sun down to Earth" in the form of controlled, thermonuclear fusion. This was the attempt to use the deuterium in ordinary water as an effectively infinite fuel supply. In only one cubic kilometer of ocean, the nuclear fusion energy that could be extracted from the approximately 1/6,500th fraction of water's hydrogen that is heavy hydrogen exceeds the combustion energy content of all the known oil reserves on Earth.

Tantalizing as the prospect of infinite energy from the oceans was, the hot fusion program had never generated even a single watt of excess power in its huge plasma reactors, which cost hundreds of millions of dollars per year to support. Success—"break-even" or "more energy out than in"—with magnetically-confined hot plasma fusion always seemed to be twenty years away. This led to the perennial joke that hot fusion is "the energy source of the future. . . and always will be." Moreover, even if the hot fusion program were to succeed in building a commercially viable central-station generator of electricity sometime in the year 2050 or beyond, the technology would have serious limitations. The energy from the hot fusion reaction of deuterium and radioactive tritium, which had to be supplied in bootstrap-fashion from the reaction, would emerge in the form of deadly neutron radiation (14 MeV neutrons). That would have to be transformed into more benign thermal energy in a hot jacket of molten lithium in order to heat water for steam-generated electricity. The practical engineering problems would be enormous, the technology would add more nuclear waste to the global inventory (though not as much as conventional fission power, or so claim the tokamak hot fusion advocates), and it was far from certain to be economically viable.

In fact, in October 1983 MIT Professor of Nuclear Engineering, Lawrence M. Lidsky, published an article ("The Trouble with Fusion") condemning the hot fusion program. It was a high-profile cover story for MIT's *Technology Review*. The stark black and white cover of the issue read, "Even if the fusion program produces a reactor, no one will want it." Other key remarks made by the outspoken Lidsky, who was then an Associate Director of the Plasma Fusion Center: "Long touted as an inexhaustible energy source for the next century, fusion as it is now being developed will almost certainly be too expensive

New Energy Times Archive

Partial Chronology of Events Relating to MIT's Handling of Cold Fusion

March 23, 1989, afternoon
Fleischmann and Pons announcement at the University of Utah.

April 17, 1989
Richard Saltus of the *Boston Globe* writes to MIT President Paul Gray complaining about lack of access to the MIT Plasma Fusion Center (see *Exhibit D* —May 1 response by MIT President Gray, *see Exhibit E,* p. 89).

April 26, 1989
MIT Professor Ronald Ballinger testifies before U.S. House of Representatives Committee on Science, Space and Technology (see *Exhibit A,* p. 84).

April 28, 1989
Professors Ronald R. Parker and Ronald Ballinger give interview to Nick Tate of the *Boston Herald*, planting anti-cold fusion story. (See *Exhibit B,* p. 86).

April 30, 1989
A late-night call by Professor Parker to Eugene Mallove's home in Bow, New Hampshire triggers press release to wire services denying the substance of the *Herald's* banner page-one story the next day. (See *Exhibit C,* p. 89).

May 1, 1989
Press release from the MIT News Office issued, which denies *Boston Herald's* characterization of Professor Parker's remarks about Pons and Fleischmann's work as "scientific schlock" and "maybe fraud." (See *Exhibit C.,* p. 89). • MIT President Paul Gray sends letter to *Boston Globe*.

June 26, 1989
MIT Plasma Fusion Center holds "Wake for Cold Fusion" party *weeks* before Phase-II calorimetry data are analyzed! (See poster, p. 76).

July 10, 1989
Section of PFC/JA-89-34 report exists which shows intermediate processed Phase-II calorimetry data. Data are not yet time-averaged. This was *not* published (see graphs, pg. 74).

July 13, 1989
Section of PFC/JA-89-34 exists which shows intermediate processed Phase II calorimetry data. Data for both H_2O and D_2O have been time-averaged in one-hour intervals. Power curve for D_2O result retains roughly the same shape as unaveraged data but has been shifted down. This *was* published (see graphs, p. 74).

July, 1989
Publication of PFC/JA-89-34 cold fusion experiments report based on work funded by DoE contract No. DE-AC0278ET51013. • Mid-July initial draft of DoE ERAB Cold Fusion Panel report is negative.

July 18, 1989
MIT PFC Director Parker's Memo on "Cold Fusion Mug" and "stamp out scientific schlock" t-shirt (see *Exhibit F,* p. 90).

November 1, 1989
Final DoE ERAB Cold Fusion Panel report is issued. It cites negative MIT PFC report— "Albagli *et al.*" as the first reference. (By contrast, positive results from U.S. Naval Surface Weapons Center are omitted.)

March 26-28, 1990
"Energy and Environment in the 21st Century" conference at MIT. MIT President Paul E. Gray makes unflattering comparison of cold vs. hot fusion (see *Exhibit G*, p. 90).

July 19, 1990
Chief Science Writer Dr. Eugene Mallove of the MIT News Office hears for the first time parts of the Parker/Ballinger/Tate interview tape played over telephone by Nick Tate of the *Boston Herald* (see *Exhibit B,* p. 86).

August 15, 1990
Meeting with Dr. Stanley Luckhardt (MIT Plasma Fusion Center) and independent scientist, electrochemist Dr. Vesco Noninski, in Dr. Luckhardt's office. Within a week Dr. Noninski is challenging the analysis of the MIT PFC calorimetry on analytical grounds.

September 8, 1990
Letter from PFC team member rejecting Noninski's analysis of the MIT experiment—letter provides minimal technical details.

October 10, 1990
Letter to Dr. Noninski from Chemistry Dept. head Professor Mark Wrighton saying "no evidence whatsoever" has been obtained to verify Pons and Fleischmann claims. Wrighton provides no technical details in rebuttal (see *Exhibit H,* p. 90).

January 16, 1991
Eugene Mallove meets with Prof. Ballinger in his office and Ballinger remarks about Pons and Fleischmann being "crooks" who could have been "locked up in jail." At Gordon Institute lecture Ballinger makes other negative remarks about Pons and Fleischmann (see *Exhibit A,* p. 84).

January 19, 1991
Mallove discovers the July 1989 down-shifted MIT excess-heat curve (See graphs, p. 74), which later became the subject of controversy.

January 25, 1991
Mallove has lunch at "Networks" in MIT Student Center with Dr. Luckhardt. Luckhardt can't explain how "bias" was taken out. Luckhardt said there could be 20 milliwatts excess power in the MIT PFC results, but "not the 80 mW that Fleischmann was talking about."

April 12, 1991
Letter from Eugene Mallove to MIT President Charles M. Vest, copy to former President Paul E. Gray, suggesting organizing an MIT panel to re-examine cold fusion in light of accumulating knowledge. No response was ever received from either MIT President (see *Exhibit I,* p. 90).

April 29, 1991
Eugene Mallove writes letter to Dr. Luckhardt requesting calorimetry information (see *Exhibit J,* p. 91).

May 13, 1991
Mallove's first call to Dr. Luckhardt to try to get MIT PFC H_2O curve.

May 20, 1991
Dr. Luckhardt cancels previously scheduled get-together with Mallove and says he forgot to get raw data at his other office. He puts Mallove off until the following Friday.

May 24, 1991
Two calls to Dr. Luckhardt (10 am and 1:30 pm)—phone messages left about getting data on H_2O curve. No response to Mallove's messages. •Near final version of Eugene Mallove's resignation letter exists.

May 29, 1991
Taping of WGBH Boston Channel 2 clip on Cold Fusion—Mallove and MIT PFC's Dr. Richard Petrasso. •Final refusal by Stan Luckhardt to turn over PFC calorimetry data.

June 7, 1991
Professor Ronald Parker publicly disparages the PFC team's calorimetry work on cold fusion! (see *Exhibit K,* p. 92) •Eugene Mallove submits his resignation from the MIT News Office (see *Exhibit L,* p. 94) following the one-hour talk on cold fusion by Frank Close at the PFC and a heated question and answer session (see *Exhibit K,* p. 92).

June 14, 1991
Eugene Mallove's request faxed to Professor Parker for promised data relating to PFC cold fusion calorimetry experiments (see *Exhibit M,* p. 99).

July 30, 1991
No response yet received from the PFC. Second request sent to Professor Parker (see *Exhibit N,* p. 100) • Press release from MIT PFC "stands by" the 1989 PFC results and conclusions (see *Exhibit T,* p. 107).

August 8, 1991
Fax letter from Parker to Mallove giving Stan Luckhardt's revised objectives of MIT PFC experiments and stonewalling again on data transfer (see *Exhibit O,* p. 100).

August 9, 1991
WBUR program about Mallove's resignation and charges airs in Boston (see *Exhibit P,* p. 101).

August 13, 1991
Fax received by Mallove from Parker with heavy water and light water curves (see *Exhibit Q,* p. 102).

August 18, 1991
Formal request by Eugene Mallove to MIT President Vest for investigation of scientific misconduct at MIT PFC, concerning both data mis-handling and deception of press and MIT News Office (see *Exhibit R, p. 102).*

September 16, 1991
Eugene Mallove responds to August 30, 1991 MIT PFC Press Release (see *Exhibit T,* p. 107).

October 9, 1991
President Vest writes to Prof. Philip Morrison requesting misconduct inquiry opinion (see *Exhibit U,* p. 108).

New Energy Times Archive

October 14, 1991
Prof. Morrison's initial inquiry report to President Vest (see *Exhibit V,* p. 109).

October 17, 1991
President Vest's response letter to Mallove (see *Exhibit W,* p. 110).

October 24, 1991
Mallove's letter to President Vest rejecting Morrison's assessment and requesting a formal investigation (see *Exhibit X,* p. 110).

November 11, 1991
Nobel Laureate Julian Schwinger speaks about cold fusion at MIT physics gathering celebrating birthday of his former student. Evidently this has no effect on Physics Dept. resistance (see pages 81-83).

December 31, 1991
Mallove's letter to President Vest asking for status (see *Exhibit Y,* p. 111).

January 2, 1992
Electrochemist Dr. Andrew Riley dies in cold fusion explosion at SRI International. Dr. Brian Ahern (an MIT graduate) tried to warn SRI of danger, but telephone call did not go through.

January 6, 1992
President Vest sends brush-off letter to Eugene Mallove (see *Exhibit Z,* p. 111).

February 9, 1991
Eugene Mallove sends new evidence of scientific misconduct to President Vest based on report of MIT graduate Dr. Mitchell Swartz's independent investigation. Mallove demands thorough investigation (see *Exhibit Z-1).* Further prompt to Vest on February 21 (see *Exhibit Z-2,* p. 113).

March 10, 1992
Dr. Luckhardt sends memo to Prof. Morrison giving further explanations of 1989 work. Redefines the objective of experiment as "turn on" of "anomalous heating event" rather than D_2O vs. H_2O comparison! (See *Exhibit Z-3,* p. 113).

March 19. 1992
NIH physicist Dr. Charles McCutchen's letter to President Vest complaining about ethical problems with MIT PFC experiment (see *Exhibit Z-4,* p. 114).

March 20, 1992
Prof. Morrison's second report to President Vest. Suggests Dr. Luckhardt continue to have possession of data and should make further assessments! (See *Exhibit Z-5,* p. 115).

April 1, 1992
President Vest's final brush off letter to Eugene Mallove giving an unacceptable conclusion. This was no April Fool joke (see *Exhibit Z-6,* p. 116).

April 2, 1992
MIT Associate Provost Sheila Widnall's letter to Dr. McCutchen—a further brush-off and statement that experimenters will continue to be processing contested data and will be writing a future memo with experiment "clarifications." (See *Exhibit Z-7,* p. 116).

May 1992
Publication of MIT PFC Technical Report (PFC/RR-92-7), a single-author (Luckhardt) "Technical Appendix to D. Albagli *et al. Journal of Fusion Energy* article" (originally 16 authors!) Error limits of MIT PFC calorimetry are further expanded and the nature of the experiment was further redefined to deflect data mishandling accusation.

July 26, 1992
Dr. McCutchen letter to Provost Widnall, asks MIT PFC to publish a correction that the experiment was not as advertised (see *Exhibit Z-8,* p. 117).

August 3, 1992
Provost Widnall's letter to Dr. McCutchen giving final MIT brush-off (see Exhibit *Z-9,* p. 117).

August 18, 1992
Dr. McCutchen letter to Eugene Mallove details his frustration with Provost Widnall's response (see *Exhibit Z-10,* p. 118).

August 19, 1991
Dr. McCutchen's final letter to Provost Widnall saying, "I am sorry MIT continues to tough it out. Apparently the university feels it need not be fair to cold fusion people." (See *Exhibit Z-11,* p. 118).

August 1992
Dr. Mitchell R. Swartz publishes fourteen page analysis of MIT PFC Phase II Calorimetry in *Fusion Facts* newsletter. Also published, in part, in subsequent*Proceedings of Fourth International Conference on Cold Fusion* and elsewhere—see references on page 77 and Jet Technology ad on p. 100.

MIT *TECHNOLOGY REVIEW* AND COLD FUSION

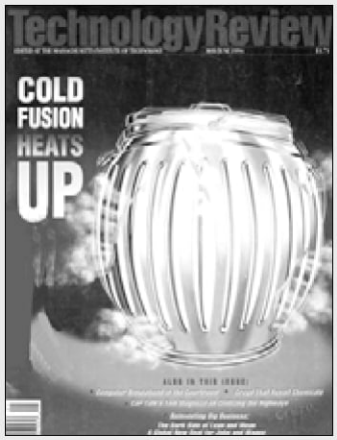
To its credit, MIT *Technology Review* published an excellent feature review article about cold fusion by Dr. Edmund Storms (Los Alamos National Laboratory, ret.) in the May/June 1994 issue. This might have been a turning point in media coverage of cold fusion, had this influential magazine continued to follow the subject. It did not.

A firestorm of protest against the Storms article had confronted then *TR* editor Dr. Steven J. Marcus, which led him to write an editorial in the August/September 1994 issue, "Don't Blame the Parent." He wrote, ". . .we'll occasionally make people angry for having allowed an author to present the 'wrong' point of view. But reaction to the cold fusion story marks the first time in my memory that dissenting readers criticized the magazine's editors not only for choosing to run this material—variously describing it as 'dreadful,' 'appalling,' 'pseudo-scientific,' 'irresponsible,' and 'an example of the goggle-eyed approach to science'—but for hurting the institutional parent in the process." Marcus heard from so-called scientists who said that the article "casts disgrace on MIT," one who said that it "trashes research at MIT," and one who wrote that it "embarrasses the Physics Department, MIT, and all graduates of MIT." (MIT students are advised to look up these articles to see for themselves what all the commotion was about.)

There were, of course, positive responses as well, which praised the editor for having found the courage to publish the Storms cold fusion article, but these did not apparently reflect the majority of the sentiments received. Marcus published six response letters in that August/September issue, including a positive one from cold fusion theorist and MIT Professor Keith Johnson and a negative letter from MIT Nuclear Engineering and Materials Science Professor Kenneth C. Russell.

Unfortunately, the protest of the Storms article in *Technology Review* was not the first time MIT faculty had become upset with *Technology Review* on the matter of cold fusion. The negative opinion of MIT Physics Professor Herman Feshbach caused the previous editor of *Technology Review*, Jonathan Schlefer, to back down in the spring of 1991 from his intent to publish *my* cold fusion review article. This 1991 article would have said essentially what Storms did in 1994, but by 1994, even more confirmatory evidence could be cited. Schlefer had *accepted* my article after much editorial revision! Both positive and negative viewpoints were presented in that approved article, plus my clearly identified opinion that the evidence was building strongly toward proof of the phenomenon. That was not negative enough for Feshbach—who called all evidence for cold fusion "junk." This sorry episode of censorship was one of the key reasons for my resignation from the MIT News Office in June 1991 (see *Exhibit K,* p. 92 for more on this event).

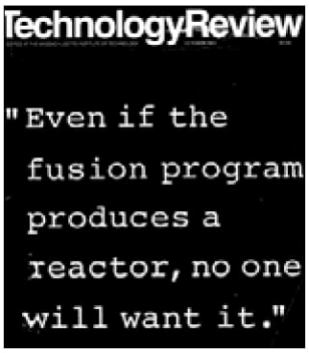
Prof. Feshbach had told me his other reason for not wanting the article to be published. He said that he had ". . .fifty years of experience in nuclear physics and I know what's possible and what's not." He later demonstrated the same sort of monumental arrogance and ignorance when he appeared on ABC Television's Nightline program, June 11, 1997. Even though Feshbach admitted that he knew absolutely nothing about the Patterson Power Cell™ cold fusion device which was the subject of the program, he told viewers that he could "categorically" state that there were no nuclear reactions occurring in it.—*EFM*



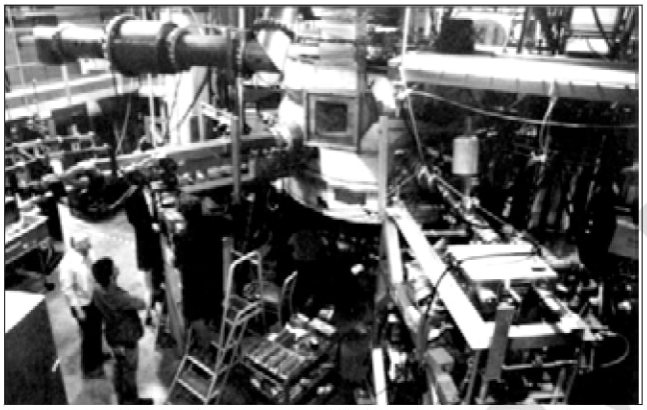
New Energy Times Archive

Professor Lawrence M. Lidsky

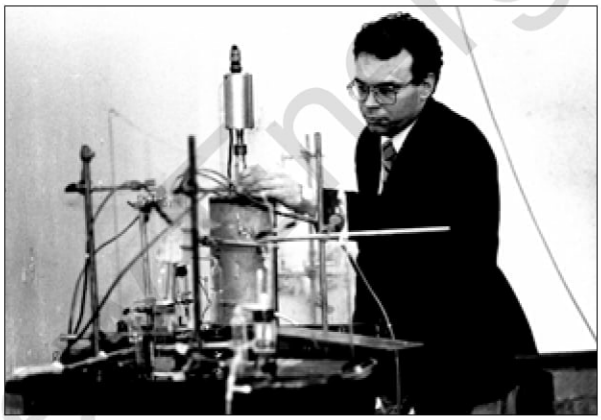
and unreliable for commercial use."; "The scientific goal of the fusion program turns out to be an engineering nightmare."; "A fusion reactor might well produce only one-tenth as much power as a fission reactor of the same size."; "The drawbacks of the existing fusion program will weaken the prospects for other fusion programs, no matter how wisely redirected." Foreshadowing the benefit of cold fusion that would emerge over five years later, Lidsky also wrote of *aneutronic* hot fusion: "Neutrons induce radioactivity and damage reactors. Neutron-free fusion might provide inexhaustible, benign power." Prof. Lidsky later moved into work at MIT on advanced fission reactors, but kept an open mind about cold fusion after it emerged.



Technology Review October 1983



Alcator-C hot fusion tokamak reactor at MIT Plasma Fusion Center. Magnetic fields confine a hydrogen plasma while the temperature and density are increased. *(From MIT Plasma Fusion Center pamphlet, "Fusion Energy Research")*



Dr. Stanley Luckhardt of the MIT Plasma Fusion Center poses in the spring of 1989 with nest of cold fusion cells on a lab cart. Ironically, this cold fusion equipment was removed from the cavernous neutron-shielded room to make way for the next generation Alcator-C hot fusion reactor. *(MIT News Office*

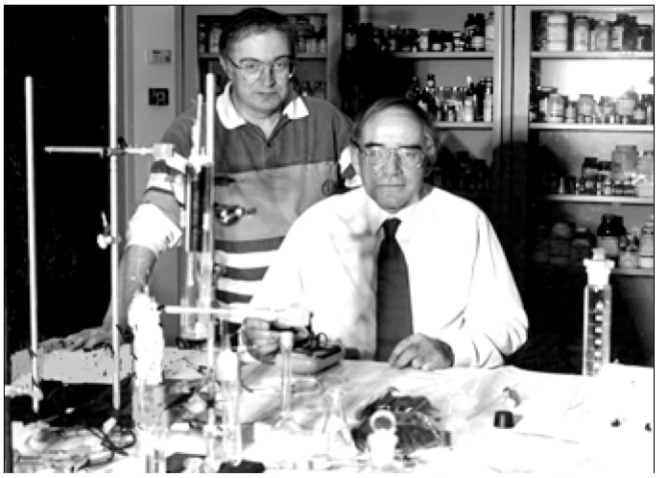
Enter Fleischmann and Pons

Onto the scene on March 23, 1989 came two world-class electrochemists, Professors Martin Fleischmann and Stanley Pons, who were boldly claiming on international television that they had already achieved break-even in some form of nuclear fusion, but in a humble jar of heavy water—without lethal attendant radiation! This was an instant prescription for controversy. By analogy, it was as shocking and insulting to the hot fusion people as if they had been told that their television set had not been able to turn on for decades because they had neglected to plug it in! The threat to the hot fusion enterprise was palpable and real. More to the point: even if the hot fusion people did not believe the Utah claims were sound, the threat that some hot fusion funding (perhaps $25 million) would be diverted by the U.S. Congress to study cold fusion was very real. The always financially embattled hot fusion program was running scared in the onslaught of cold fusion news.

MIT Professor Ronald Ballinger, who would play a key role in the scandalous attacks against cold fusion, testified before the U.S. House of Representatives' Committee on Science, Technology, and Space (See *Exhibit A*, p. 84). His April 26, 1989 testimony had a seemingly appropriate "wait and see" message, but behind the scenes Ballinger, Parker, and other MIT hot fusioneers had among themselves already dismissed cold fusion. They were sharpening their knives against Fleischmann and Pons (See recorded interview with *Boston Herald*, *Exhibit B*, p. 86).

The idea that deuterium in heavy water might be undergoing some kind of nuclear fusion reaction within the palladium cathodes of the Pons-Fleischmann cells was, of course, very difficult to accept. Where was the expected lethal radiation, for example, which standard nuclear physics would seem to predict? Why weren't Pons and Fleischmann dead if they had truly generated even a minor fraction of a watt of cold fusion-derived energy? This became known as "the dead graduate student" problem. Furthermore, how could the palladium cell have overcome the natural, very high electric repulsion force between the positively charged deuterium nuclei—the so-called Coulomb barrier that had been thought to put an absolute barrier between high energy nuclear physics and ordinary chemistry? Elements (except those that are radioactive or which spontaneously fission) should retain their identities. This is basic scientific "fact" doled out in high school science classes. Room-temperature fusion of even *light* elements such as hydrogen or lithium was considered to be prima facie impossible. (There was an old pre-cold fusion era joke among MIT students about the need for a Department of Alchemy, but MIT was apparently not quite ready for the real thing!)

Conventional understanding was that nothing at ordinary conditions could bring the nuclei of deuterium close enough together such that the nuclear forces, at very close approach, would take over and facilitate fusion to helium—or to anything



Professors Martin Fleischmann (R) and Stanley Pons (L).
Infinite Energy archives

Not for Sale Archive

else. That *two* "miracles" were implicit in cold fusion was just too much to bear for the mainstream physics community. Nonetheless, the establishment held its skepticism in check—at least publicly—for several weeks. Some scientists told the news media that the claims were "very interesting," but they thought they were unlikely to be true. By implication, they suggested there might be a mistake, which they would likely find after doing their experiments to check up on Fleischmann and Pons.

Immediately, the cold fusion story became very big news all around the world. Thousands of scientists and basement inventors tried to verify—or disprove—the claims from Utah. The May 8, 1989 editions of *Time*, *Newsweek*, and *Business Week* ran prominent cover stories on cold fusion—a first for science cov-

erage apart from events in space exploration. The question of the hour was—as *Business Week* editorialized on its cover: Is cold fusion a "miracle or a mistake"? Of course that was the possibility that had to be excluded—a major mistake in either excess heat measurement or nuclear measurements.

When cold fusion was announced, I had the good fortune to be the chief science writer at the MIT News Office, the main public relations arm of MIT. My tenure was from September 1987 through June 1991. Previously, I had written major scientific articles for MIT *Technology Review*, the magazine of my alma mater's Alumni/ae Association. After leaving my job as an aerospace engineer at MIT Lincoln Laboratory in 1985, I shifted careers and had worked as a science writer and broadcaster for the Voice of America in Washington, DC. I would also eventually teach science journalism both at Boston University and at MIT in the Department of Humanities (both when I was in the MIT News Office, and for a time afterward).

My position at the News Office required me to interact daily with members of the national and international press. Thus, when the Pons and Fleischmann announcement occurred, it was my job to report to the media what certain key scientists at MIT were thinking about the amazing claims out of Utah.

I had already been instrumental, some weeks before March 23, 1989, in exposing the entire science writing staff and senior editors of *The Wall Street Journal* to the hot fusion program at MIT, where the Alcator line of tokamaks were being developed. I did that proudly. In fact, I remember introducing Plasma Fusion Center (PFC) Director Ronald R. Parker to the *Wall Street Journal's* Jerry Bishop, the senior reporter who would later write an award-winning series of articles on cold fusion. As an engineer turned writer-engineer, I had been since age sixteen an advocate for hot fusion.

While a student in engineering at MIT in 1967, I remember being impressed by the Russian hot fusion exhibit at the world Expo in Montreal. I thought that hot fusion offered a real though difficult-to-develop solution to the world's energy needs. Because I had been trained as an aerospace engineer with a particular interest in interstellar propulsion methods, I was fond of hot fusion, because it might offer a very high performance propulsion system for limited travel to the "local" stars. I would write of this in my 1989 book, co-authored with colleague Dr. Gregory Matloff, *The Starflight Handbook: A Pioneer's Guide to Interstellar Travel* (John Wiley & Sons). In 1969

I had written a lengthy term paper for MIT course 16.53 on the Bussard Interstellar Ramjet concept, which used the hydrogen of the interstellar medium as fusion fuel. In the 1970s and 1980s, I collaborated with physicist Robert L. Forward of Hughes Research Laboratories on lengthy bibliographical studies of the related subjects of advanced interstellar propulsion concepts and the search for extra-terrestrial civilizations (SETI).

The cold fusion story quickly drew very heavy media attention, and I was rapidly drawn into the frenzy that resulted at the MIT News Office. There were many requests for interviews with Ronald Parker and others at the PFC. A group of PFC and Chemistry Department scientists and students had immediately set out to check the Utah claims. There were regular calls to me at the News Office to provide status reports, photo opportunities, and interviews for members of the PFC team. Then in mid-April 1989, Professor Peter L. Hagelstein, a laser and quantum physics

Massachusetts Institute of Technology
TECH TALK

September 16, 1987
Volume 32, Number 7

Dr. Eugene Mallove '69 named News Office science writer

Dr. Eugene F. Mallove '69, an engineer and scientist who has written widely on science for the Voice of America, *The Washington Post* and *Technology Review*, has been appointed chief science writer for the MIT News Office. The appointment, as assistant director, was announced by Kenneth D. Campbell, director of the News Office. "Gene Mallove brings three great strengths to the News Office: his background in science and engineering; his MIT experience; and, most importantly, his ability to communicate his fascination for science, both in the written word and on the air-waves. I am delighted to welcome Dr. Mallove back to MIT," said Mr. Campbell.

A science writer for the past five years, Dr. Mallove's most recent position was as international science writer and broadcaster at the Voice of America, which he joined in 1985. He was responsible for a weekly 15-minute "New Horizons" program on science, technology, and medicine, and for a daily five-minute program of science teaching to the world, "Science Notebook."

He has written free-lance articles for the *Washington Post* and other newspapers, and for *Technology Review* and a new magazine, "Computers in Science." He is the author of *The Quickening Universe*, to be published by St. Martin's Press this fall.

Dr. Mallove received his SB in aeronautical and astronautical engineering from MIT in 1969, and his SM in the same field in 1970. In 1975, he received from Harvard University his ScD degree in environmental health sciences, specializing in aerosol physics and air pollution control.

His career in science and engineering includes work as a consulting astronautical engineer on space propulsion systems with Hughes Research Laboratories, 1970-77; engineer with The Analytical Science Corporation, 1977-79, and with Northrop Co. (Precision Products Division), 1980-81; systems engineering manager with Jaycor, Systems Engineering Division, 1981-82; and engineer with MIT Lincoln Laboratory, 1983-85.

He founded a firm, Astronomy New England, Inc., which developed and marketed astronomy-related products for six years, ending in 1985. (Reprinted from *MIT Tech Talk*)

New Energy Times Archive

expert in the MIT Department of Electrical Engineering and Computer Science, went public with a theory of how cold fusion might be explained in terms of "coherent nuclear reactions." Professor Keith H. Johnson of the MIT Dept. of Materials Science, another MIT luminary, with deep knowledge of palladium hydrides and superconductivity in his background, also put forth a theory that allowed nuclear reactions to occur in the Pons-Fleischmann cells. Unlike Hagelstein, who proposed pure nuclear reactions operating in a coherent fashion with a metal lattice, Johnson tried to explain the excess heat as a result of peculiar effects of so-called Jahn-Teller chemical bonding. I thought this was a wonderful honor for MIT, to have two open-minded theorists approaching the Utah results with caution, but attempting to pose explanations for it if it could be confirmed. Others at MIT did not hold this view. The Hagelstein-Johnson work was almost immediately regarded with disdain—particularly by the plasma fusion people (see MIT Provost John Deutch's remarks, p. 118). So there were early-on two camps at MIT, one largely negative (but at that point generally restrained in its public comments), and another putting forth hope that the Utah discovery was no mistake and could be explained on theoretical grounds—much as Nobel Laureate Julian Schwinger began to try to do at that time. (See Julian Schwinger's talk on cold fusion, which he delivered at MIT in November 1991 pp. 81-83.)

The story was growing more fascinating every day, as reports of positive results in replication efforts came in from around the world, as well as news of negative results from other laboratories. I was able to write a series of articles* about MIT's response to cold fusion for *MIT Tech Talk*, the administration newspaper that circulates on campus and is used as a public relations tool to influence the general mass media. (*Tech Talk* is not to be confused with the MIT student newspaper, *The Tech*).

There were also a few immediate false-positive results from outside MIT, such as from Georgia Tech, that were reported prematurely to the press. These left several scientists embarrassed when they had to retract or sneak away with red faces—as Charles Martin did at Texas A&M University. Unfortunately, these and other political issues came to color the attitude of many observers of the cold fusion scene—especially because Pons and Fleischmann had been forced to make their announcement via a press conference, rather than through scientific publication. The reasons for the press conference are too involved

* April 5, 1989, Vol. 33, No. 27, "Recent 'cold fusion' claims baffle experts."
April 26, 1989, Vol.33, No. 29, "Cold fusion: theories, controversy abound."
May 3, 1989, Vol. 33, No. 30, "Group finds flaw in cold fusion experiment."
May 31, 1989, Vol. 33, No. 34, "Cold fusion is still a hot topic."

MIT Prof. Peter L. Hagelstein in his laser laboratory at MIT, circa 1994.
(*Photo by Eugene Mallove*)

MIT Prof. Keith H. Johnson at home in his movie production studio.
MIT News Office

Office of the President

May 6, 1988

Mr. Eugene F. Mallove
Room 5-111

Dear Gene,

I write, a bit belatedly, to express my appreciation to you for your tremendous help this spring in preparing those remarks on scientific illiteracy. As you know, I have gotten considerable mileage from them already (the Washington campaign opening, the New York Academy, and the Time-Life editors) and will get even more mileage from them in the months ahead. Your collaboration in this task was a tremendous help, and I am much in your debt.

I suspect you were startled, as was I, by the revelations this week about the Reagans' reliance on astrology. On second thought, I guess we should not be surprised.

With warmest regards and best wishes,

Sincerely yours, Paul E. Gray PEG/mmd cc: Kathryn W. Lombardi

Eugene Mallove was held in high regard by many members of the MIT faculty and administration, as this 1988 letter from Dr. Gray attests.

Dr. Eugene F. Mallove's Input for MIT President Paul Gray's Remarks for MIT Technology Day 1989 (Prepared, May 11, 1989)

Two decades ago humanity was poised to begin an epic journey—the first manned landing on another celestial body, the Moon. We well recall the extraordinary contributions made by the thousands of scientists and engineers to the epic flight of Apollo 11 in July, 1969—in particular the work of MIT engineers who developed the guidance and navigation systems for our spectacularly successful lunar missions.

We remember the electric atmosphere and the spirit of global celebration—even amidst domestic conflict and war—as the world in rapt attention watched a tiny contingent of humanity make a giant leap onto a new world. And we remain very proud that a son of MIT who received his doctorate from the Department of Aeronautical and Astronautical Engineering, Col. Edwin E. Aldrin, Jr., was aboard Apollo 11 and became the second human being to step onto the lunar surface.

The launching of the first Sputnik in 1957 is often cited as the beginning of the space age. The era from 1957 through the watershed year of 1969—incidentally, the time of graduation of the 100th MIT class—contrasts remarkably with the present. Slide rules were still the indispensable companions of scientists and engineers; desk-top computers were unheard of and children did not play at video games; energy crises were only a theoretical possibility—as was the threat of global climate change; environmental issues were yet to rise to the fore of public attention; genetic engineering had not entered the public lexicon; and nuclear power seemed to have an endless, promising future.

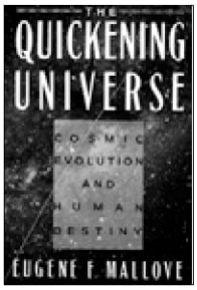
Today we aim to go beyond the Moon, not only with our space program, but with our ambitions to make a better planet Earth through science and technology. But these ambitions are beclouded by a worrisome anti-technology backlash and pervasive scientific illiteracy in our society that threatens not only our venture into space and efforts to reach a reasonable accommodation with nature, but our very standard of living. As MIT's recently released "Made in America" study suggests"...

New Energy Times Archive

to explore here, although Dr. Fleischmann himself sheds some additional light on the topic in an essay in this issue (see pp. 25-28). However, it is a matter of record that Fleischmann and Pons really did not want to make their disclosure for another eighteen months until they understood their discovery better. The parallel claims by physicist Steve Jones of nearby Brigham and Young University, patent issues, and other conflicts brought the issue into public view in March 1989. Further complicating the story and enraging other scientists, lawyers at the University of Utah prohibited or retarded the disclosure of experimental details by Fleischmann and Pons.

As a historian of this subject, I feel confident in stating that if Fleischmann and Pons had been allowed to hand out at their press conference the pre-print of their paper which was later that spring published in the *Journal of Electroanalytical Chemistry*, the intensity of opposition to cold fusion would have been reduced by at least fifty percent.

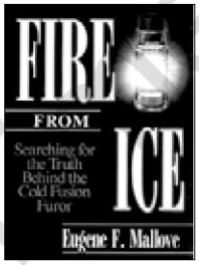
If one had any interest in the process of science, this was already a first-class captivating story. Naturally, I called my literary agent at the time, Richard Curtis, and alerted him that there might be a new book for me in this saga. Because I had already written two books (at the time I was still completing *The Starflight Handbook*), it was not difficult to convince John Wiley & Sons to offer me a contract for a book on cold fusion. I didn't know how the story would turn out, but it was certainly going to be a matter of some interest given the already huge media coverage. My first book was *The Quickening Universe: Cosmic Evolution and Human Destiny* (St. Martin's Press, 1987), which had been published soon after I arrived at the MIT News Office.



One of the stipulations in the cold fusion book contract was that if *Nature* or *Science* magazine (or both) were to reach the general editorial conclusion that cold fusion was not real, the publisher could revoke the contract. As would transpire, that happened; the contract was revoked. In the spring of 1989 and beyond, the complex politics among the hot fusion program, the Department of Energy ERAB Cold Fusion Panel, the cold fusion camp, the media, and the mainstream science community led to widespread rejection of cold fusion as a Big Mistake—incompetence on the part of Pons and Fleischmann and others reporting positive results, or worse. "Possible fraud" and "scientific schlock" is how PFC Director Ronald Parker would characterize Pons and Fleischmann's work to *Boston Herald* environmental reporter Nick Tate in an interview in late April 1989, which surfaced on May 1st. That May day in Baltimore, the absent electrochemists were viciously attacked at the meeting of the American Physical Society. The "F-word"—fraud—had been unleashed against cold fusion, thanks in no small way to the MIT PFC. *Boston Herald* Reporter Nick Tate would later write in a retrospective (June 8, 1991): "The MIT analysis debunked the Utah claims, and in an interview with the *Herald*, Parker—who wrote the report with Dr. Richard Petrasso—said the chemists misinterpreted their results. He also called it possibly fraudulent 'scientific schlock.' Some say those comments set the tone for the national criticism of the Utah work that followed."

Nick Tate, then of *Boston Herald*, now with *Atlanta Journal Constitution*
(Photo courtesy of AJC)

But as we all know, the cold fusion story did not die. Positive results, as well as negative results in attempts to replicate the Pons and Fleischmann experiment, continued to be reported through 1989 and beyond. I was fascinated by the trend, not knowing how it would all come out. I was trying to be as objective as possible within the tumult. Certainly, I was encouraged by much of what I heard, but I was also discouraged by what my contacts at the MIT Plasma Fusion Center were saying. Some of them, such as Dr. Stan Luckhardt, told me that the tritium detection in cold fusion experiments at Los Alamos National Laboratory should be ignored because it had been done by "third-rate scientists." I assumed, provisionally, that these MIT experts knew what they were talking about. These were Dr. Edmund Storms and Dr. Carol Talcott—in retrospect definitely not "third rate." Despite *Nature* and *Science* magazines' negativity, eventually the sharp editor at John Wiley & Sons, David Sobel, perceived that it would be a good idea to reinstate the book contract, so I continued to follow the story. Even without the contract, I would have continued to be deeply immersed in the field. How could any serious person with a strong science background not be, so intriguing had become the physical evidence—and, in parallel, its public rejection. And several MIT professors remained very interested in it—not only Peter Hagelstein and Keith Johnson, but Prof. Louis Smullin, Prof. Lawrence Lidsky, Prof. Donald Sadoway (who filed a patent too!), and Prof. Philip Morrison.



In May 1991, *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor* came out. Its general conclusion was that the evidence for cold fusion was overwhelmingly compelling. In my view, for four or five years now, the basic evidence has been 100% confirmed; it is not merely compelling. Commercial opportunities abound for engineering power-generating reactors, even though the precise microphysical characterization of "cold fusion" remains contentious. In 1991, Julian Schwinger offered this promotional comment for the jacket of *Fire from Ice*: "Eugene Mallove has produced a sorely needed, accessible overview of the cold fusion muddle. By sweeping away stubbornly held preconceptions, he bares the truth implicit in a provocative variety of experiments." (See page 80 for further positive comments on *Fire from Ice* by Schwinger and other MIT-affiliated people.)

Julian Schwinger
Infinite Energy archives

In 1991, I thought that both cold fusion and hot fusion could play a complementary role in the energy economy of the world—even though neither technology had been developed to the stage of commercial devices. I offered that opinion in *Fire from Ice*. But I was on dangerous ground. That was the last thing that the hot fusion people wanted to hear! They thought they had buried cold fusion about two years before. They had been fighting cold fusion in the press and in government from the outset.

Today, it is hot fusion that will be buried. Once the first commercial prototype reactors using cold fusion get wide-spread public acceptance—and they inevitably will—the white elephant of the tokamak hot fusion program is likely to be abruptly canceled by an outraged Congress. The U.S. DoE-Academia scandal against cold fusion demands a Congressional investigation if ever a matter of pressing scientific, technological, and legal importance did. Congress has already killed U.S. involvement in the $10-billion ITER (International Thermonuclear

New Energy Times Archive

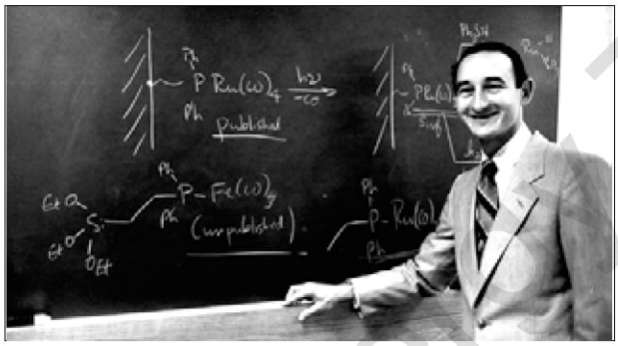
Experimental Reactor). The media, in general, are still largely ignoring scientific and commercial developments in cold fusion, but commercial-scale reactors will be impossible to deny— even for some heretofore obtuse science journalists who should have been continuing their coverage had they not been so strongly influenced by the likes of the negativists at MIT and elsewhere.

It Began at MIT

By the spring of 1991, most of the media and the certainly the vast bulk of the scientific establishment had written off cold fusion. Fortunately for us all, they were—and are—all wrong. How did the scientific community and the media get the idea that cold fusion was bunk, "pathological science," and worse, when experiments continued worldwide? Substantial, increasingly refined experimental proofs were published—even in peer-reviewed journals, but the goal posts kept being moved by the opposition. Today, these goal posts are so distant, they are off the planet.

In retrospect, I have concluded that much of the blame for the "cold fusion war"—and it certainly has been just that—stems from a vituperative campaign against the field with deep roots at MIT, specifically at the MIT Plasma Fusion Center. Not exclusively in that lab, however. Then chemistry Professor Mark S. Wrighton was also on the team that was investigating cold fusion. He later signed the infamous rush-to-judgement report against cold fusion by the U.S. Dept. of Energy (Prof. Mildred Dresselhaus of MIT also signed the negative DoE report but was much less involved, and as of 1999 is apparently "neutral" about cold fusion. One wonders about the propriety of her public silence). Wrighton became Provost of MIT in (1990) after Charles Vest (formerly of the University of Michigan) became President of MIT and picked him. Since 1995, Wrighton has been Chancellor at Washington University in St. Louis.)

Prof. Mildred Dresselhaus
MIT News Office



Prof. Mark S. Wrighton, then head of the MIT Chemistry Dept., signed the U.S. Dept. of Energy negative cold fusion report, which relied heavily on later contested results from the MIT PFC-Chemistry Department team.
Photo: MIT News Office

In the spring of 1991, as I was finishing *Fire from Ice*, and feeling increasingly uncomfortable with what was happening at MIT with respect to cold fusion, I made a fateful discovery. Questions had already arisen about exactly how the MIT PFC-Chemistry Dept. team had analyzed their excess heat calorimetry study that compared a heavy water/palladium cell with an ordinary water/palladium cell. This was the so-called "Phase-II Calorimetry" study that had been published in the *Journal of Fusion Energy*. (Edited at the MIT Plasma Fusion Center—how's that for short-circuiting peer review!) From the pile of information that I had been collecting about the on-going work at MIT and elsewhere, I found two draft documents concerning this calorimetry that had been given to me by PFC team members during the rush toward publication. I could see immediately that there was a serious discrepancy between the unpublished, pre-processed raw data (the July 10, 1989 draft) and the final published data on the July 13, 1989 draft. (See adjacent page of graphs reproduced from these drafts). At first glance, it appeared that the data had been altered between July 10th and 13th to conform to what would be most welcome to the hot fusion people—a null result for excess heat in the heavy water data. I would later publicly challenge the creation and handling of these graphs by MIT PFC staff (see extensive *Exhibits J* through Z-11).

The Phase-II Calorimetry curves were later investigated in the outstanding analysis by my cold fusion colleague and fellow MIT graduate Dr. Mitchell R. Swartz. There can be no doubt now that these curves were the end result of a serious lapse in scientific standards in this affair that happened at MIT.

Our alma mater, which had played such a critical role in the development of radar in World War II, in the Apollo flights to the Moon, in deep space missions, in electronics, in biotechnology, in the chemical industry, in defense systems, and too many other fields to mention in one sentence, would acquire the reputation in the media as the "bastion of skepticism" against cold fusion. Tragically, MIT as an institution was not to fulfill the role it could have played in bringing cold fusion technology to the world. Quite the contrary, thanks to various false information coming from the hot fusion lab at MIT, the high-profile reputation of MIT was used to legitimize the view that cold fusion is bunk. It was said that the PFC calorimetry results disproved cold fusion—showed no excess heat. This is far from correct, as Dr. Swartz admirably showed. His analysis has been published in several venues (see references, page 119).

THE WALL STREET JOURNAL WEDNESDAY, APRIL 15, 1992

TECHNOLOGY

Physicist to Report Cold Fusion Findings From Japan at MIT's Bastion of Skeptics

By Jacob M. Schlesinger

soft-spoken, graying nuclear physicist says

JOURNAL OF FUSION ENERGY

Journal of Fusion Energy, Vol. 9, No. 2, 1990

Measurement and Analysis of Neutron and Gamma-Ray Emission Rates, Other Fusion Products, and Power in Electrochemical Cells Having Pd Cathodes

David Albagli,[1] Ron Ballinger,[3,4] Vince Cammarata,[1] X. Chen,[2] Richard M. Crooks,[1] Catherine Fiore,[2] Marcel P. J. Gaudreau,[2] I. Hwang,[3,4] C. K. Li,[2] Paul Linsay,[2] Stanley C. Luckhardt,[2] Ronald R. Parker,[2,5] Richard D. Petrasso,[2] Martin O. Schloh,[1] Kevin W. Wenzel,[2] and Mark S. Wrighton[1,5]

Results of experiments intended to reproduce cold fusion phenomena originally reported by Fleischmann, Pons, and Hawkins are presented. These experiments were performed on a pair of matched electrochemical cells containing 0.1 x 9 cm Pd rods that were operated for 10 days. The cells were analyzed by the following means: (1) constant temperature calorimetry, (2) neutron counting and γ-ray spectroscopy, (3) mass spectral analysis of ^{4}He in effluent gases, and ^{4}He and ^{3}He within the Pd metal, (4) tritium analysis of the electrolyte solution, and (5) x-ray photoelectron spectroscopy of the Pd cathode surface. Within estimated levels of accuracy, no excess power output or any other evidence of fusion products was detected.

KEY WORDS: Fusion; cold fusion; palladium; excess heating.

The New York Times

Cold-Fusion Claim Is Faulted On Ethics as Well as Science

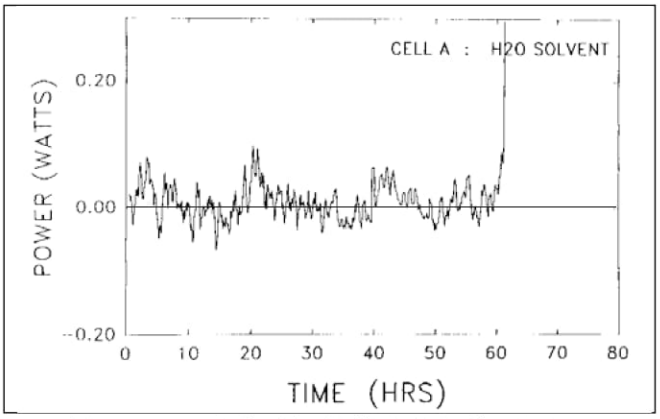
By WILLIAM J. BROAD

The startling assertion by two ... ing work on the atom.

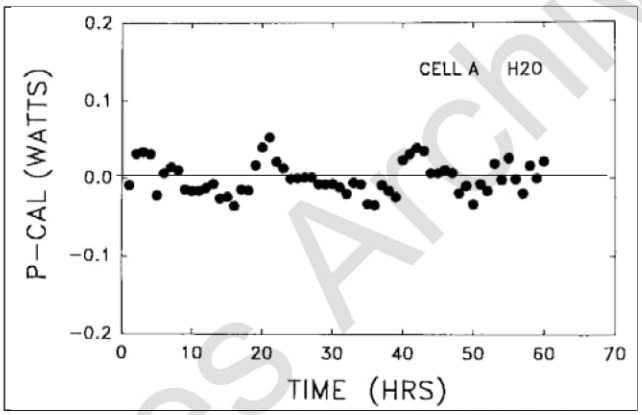
From the vantage point of 1999, the role of the MIT Plasma Fusion Center/Chemistry Department team that investigated the cold fusion claims in 1989 grows clearer. It is a sad tale that cannot be fully addressed in a short space. Suffice it to say that early on, senior members of the PFC/Chemistry group, such as Dr. Richard Petrasso and Prof. Ronald Parker, took the view that the Utah claims were flawed, or worse, fraudulent. It went downhill from there. In William Broad's 1991 front-page news story in the March 17, 1991 Sunday *New York Times*, senior PFC physicist Richard Petrasso revealed his original views about Pons and

New Energy Times Archive

The two *pairs* of graphs (below), referring to the same experiment, are from *two* drafts (executed three days apart) of the MIT PFC Phase-II Calorimetry comparative study of a heavy water (D_2O) Fleischmann-Pons cold fusion cell and an ordinary water (H_2O) control cell. In the July 10, 1989 draft, there is clear evidence of excess heat (beyond electrical input power) in the D_2O cell, but no visually apparent excess in the H_2O cell. The data were averaged over-one-hour intervals to produce the July 13, 1989 draft, which shows no excess heat in the D_2O cell. There is now no doubt that to produce the July 13, 1989 draft, the D_2O data had to be treated differently than the H_2O data to give the final impression of a "null" result—no excess heat for D_2O. The results were published in this form in the *Journal of Fusion Energy* and a MIT PFC Tech Report, widely cited (especially by DoE) as evidence that the Fleischmann and Pons claim was false. In essence, the hour-averaged data were properly transformed from the intermediate processed form (July 10) for the H_2O control experiment, but the D_2O experiment curve in the July 13 draft appeared to be arbitrarily shifted down to make the apparent excess heat vanish. There is no justification for this curve shifting. The manipulation of the data between dates July 10 and 13 was more disturbing and unexplained, because the two sets were "asymmetrically" treated, as proved in the extensive analysis done by MIT graduate Dr. Mitchell R. Swartz.



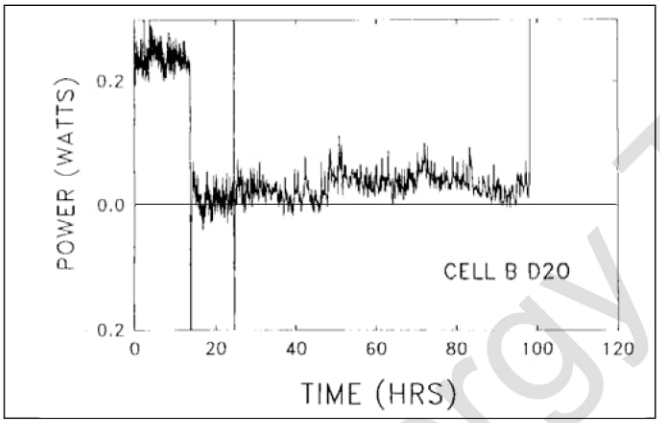
Excess Power Data. July 10, 1989 H_2O Unpublished.



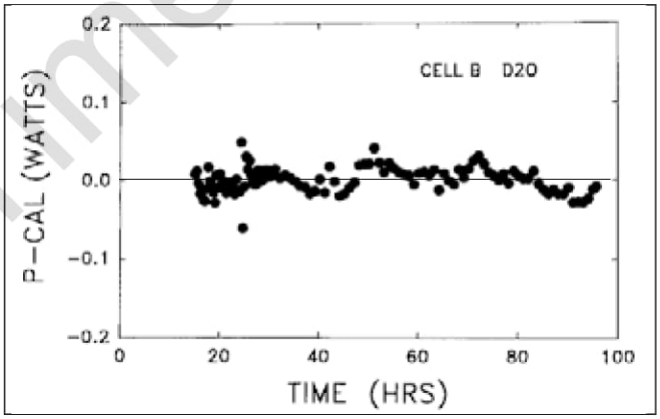
Excess Power Data. July 13, 1989 H_2O Published

Unpublished data

Published data



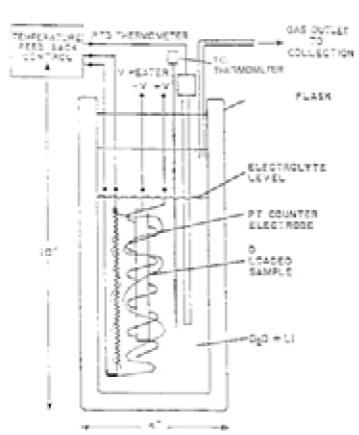
Excess Power Data. July 10, 1989 D_2O Unpublished.



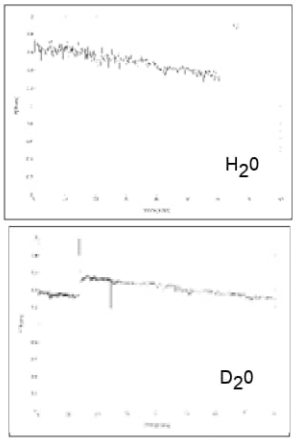
Excess Power Data. July 13, 1989 D_2O Published.

How the MIT PFC Experiment Worked—Or Didn't!

Accurate calorimetry of electrolytic cells is a difficult task, prone to many subtle errors, which crept into the 1989 MIT PFC Phase-II Calorimetry experiment. A schematic diagram of the experiment is at the right. A temperature sensor monitors the temperature of the water. Auxiliary heater power is automatically adjusted to maintain constant cell temperature, so the heater power is a measure of the energy released in the cell. Thus, if heat is generated within the cell, less heater power is required. However, water is lost from the cell during the experiment, reducing the ease with which heat is conducted to the environment, which also tends to reduce the heater power requirement. During the experiment, the input power shows a declining heater power trend from water loss. The graphs above have been compensated for this water-loss trend. "Compensation" is error prone, especially where the heat release (possible cold fusion power) may be steady. The MIT researchers *later* (after their report was challenged) said they expected a "sudden turn on" of excess heat. Dr. Swartz concludes that "The Phase -II methodology is flawed because it masks a constant [steady-state] excess heat." He also notes, ". . .the PFC data itself indicates that evaporation was a minor source of solvent loss...most solvent loss occurred by electrolysis. Such solvent loss would be greater for the H_2O solution...such electrolysis is used commercially to isolate heavy water...putative differential excess solvent loss for heavy water is not a reasonable explanation for the asymmetric algorithm used to shift the 7/10/1989 D_2O curve." On June 7, 1991, Prof. Ronald R. Parker publicly stated that data from the MIT PFC was "worthless," yet it had been published in a fusion journal edited at MIT. Later in 1991, he said that he stood by the negative conclusions of the 1989 MIT PFC report. What are we to believe?



Schematic of PFC Experiment
From Draft MIT PFC Report



Heater power decline.
MIT PFC Raw Data

New Energy Times Archive

Fleischmann: "I was convinced for a while it was absolute fraud. Now I've softened. They probably believed in what they were doing. But how they represented it was a clear violation of how science should be done." This is final proof, as though more were needed, that the scientific experiments to investigate cold fusion were inappropriately biased from the outset.

Petrasso's comments came within Broad's article, bannered with "Cold-Fusion Claim is Faulted On Ethics as Well as Science." The article amounted to a virtual promotional book review of UK physicist Frank Close's book, *Too Hot to Handle*, which came out at about the time of *Fire from Ice*. *The New York Times* also reviewed Close's book in its Book Review section. Curiously, *Fire from Ice* was never reviewed by the *Times*. Frank Close, who worked closely with Petrasso *et al.* in assaulting Pons and Fleischmann, falsely accused them of having fudged gamma-ray spectroscopy data. The bizarre truth is that even had Pons and Fleischmann faked gamma ray data—they most certainly had not—their all-important nuclear-scale excess power results, the key signature of cold fusion, has withstood the test of time. Cold fusion is now being developed commercially. To their credit, Fleischmann and Pons were not comfortable with the preliminary nature of their neutron/gamma-ray data and have long since withdrawn those data. Others subsequently confirmed much lower levels of neutron emission. On the other hand, the use of the strawman of gamma-ray curves by Petrasso *et al.* at the MIT PFC is all the more reprehensible when the history of *real* data fudging in cold fusion is examined—the data "processing" (*i.e.* improper manipulation) of calorimetry curves from electrochemical experiments performed at the MIT PFC in the spring of 1989.

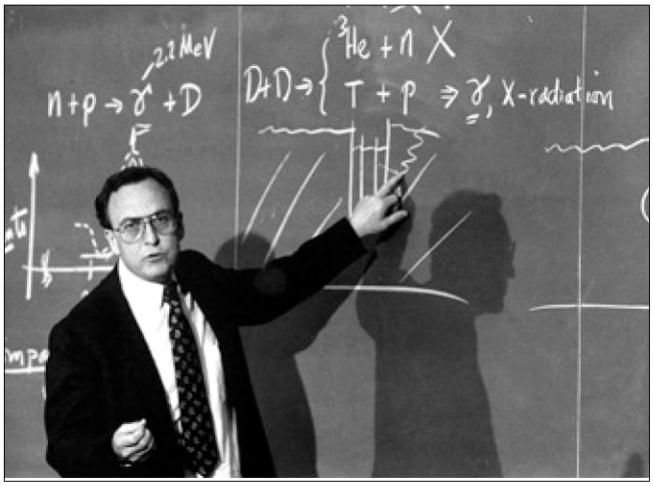
The energy and environmental future of the world hung in the balance—and the MIT PFC people failed us. They preferred to get rid of a scientific claim in which they did not believe, and which threatened their federally-funded program, by playing politics with the media, trivializing their experiments, and ultimately foisting on the world highly flawed data—some would say fraudulently represented data—from a calorimetry experiment ostensibly performed to determine scientific truth. . .

Let us not forget, these were serious experiments, funded by the U.S. Department of Energy under Federal contract. The authorization to investigate came from U.S. President George Bush through Energy Secretary Admiral Watkins. (As a general matter, people who file false reports to Federal agencies are subject to criminal sanctions if this work is brought to the attention of appropriate investigative authorities before the statutes of limitation expire.) This calorimetry issue was not a small matter. In the spring of 1989 it was absolutely critical to determine whether there was anything to the Pons and Fleischmann claims. The energy and environmental future of the world hung in the balance—and the MIT PFC people failed us. They preferred to get rid of a scientific claim in which they did not believe, and which threatened their federally-funded program, by playing politics with the media, trivializing their experiments, and ultimately foisting on the world highly flawed data—some would say fraudulently represented data—from a calorimetry experiment ostensibly performed to determine scientific truth.

To understand how the curves that I and later Dr. Swartz analyzed in his report came about, one should have some background. In late April 1989, Professor Parker and Professor Ronald Ballinger, both members of the PFC team then investigating the claims of Pons and Fleischmann, held a covert interview with *Herald* reporter Nick Tate to plant a very negative story against the Utah work. No one at the MIT News Office was told of this interview until late on the night before the story was to appear in banner headlines in the *Boston Herald*. As Parker told Tate (a tape released by the *Herald* confirms this—see *Exhibit B*), Parker and Ballinger *et al.* were opposed to the "cheer-leading" for cold fusion by the *Boston Globe*. They wanted to give Tate an exclusive story about some nuclear physics evidence that they said they had developed, which they claimed would prove the Fleischmann-Pons experiments to be highly flawed. This evidence concerned the gamma-ray spectra coming from attempts to measure neutrons impinging on a water bath near the Pons-Fleischmann cells.

Historically, it is evident that this *Herald* story helped unleash the tidal wave of negativity against Fleischmann and Pons and others who continue to work in the field. Ironically, Parker *et al.* accomplished what they really set out to do with that story, but at the time Parker attacked reporter Tate for *allegedly* mis-reporting what he had said during his interview. Tate came very close to being fired on the spot by his editor; *he would have been fired* had he not had an audio tape of the interview to confirm what he had been told by Parker. After all, it was an MIT professor's word against that of a young reporter.

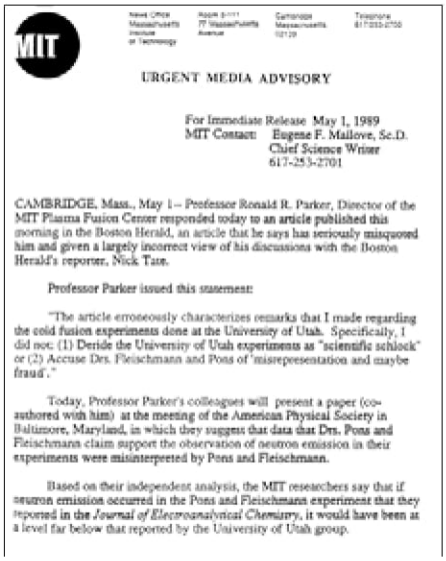
A frantic Ronald Parker, perhaps fearing that he would be sued by Pons and Fleischmann for the harsh words that were quoted a bit too explicitly for his taste, called me late on the night of April 30, 1989. He had me dispatch a press release to



Prof. Ronald R. Parker, at hastily called MIT press conference on May 1, 1989, explains what the MIT PFC team believed to be the errors in the Fleischmann-Pons neutron measurements. He also denied using the harsh language against Fleischmann and Pons quoted in the *Boston Herald* story that day. *Photo: MIT News Office*

New Energy Times Archive

the wire services denying the impending *Boston Herald* story, the exact nature of which he had learned from a call from CBS television. Of course, I had at that time no reason to doubt what he was telling me: that the story was a distortion. I would learn the stark truth about this deception only over a year later when Tate allowed me to listen to the actual tape. There can be no denying what Parker told Tate about Fleischmann and Pons. In one key passage in the interview, Parker says this: "So, what are you going to do with this, uh, Nick? You know this is. . . what you are hearing is that we think it's a scam, right?" Tate responded: "Why is it today that you think it's a scam?" Parker's reply: "We have been studying the evidence together very slowly and we want to have a paper out on this before we actually blast them. Monday (May 1, 1989) we're putting a paper out on it. . ." In addition to this, the actual word "fraud," was used by Parker no less than five times on the audio tape—as he discussed Pons and Fleischmann's work. This tape is a key "smoking gun" of the entire cold fusion controversy. The treachery and conniving by Parker and Ballinger are there for all to see—disgraceful!



MIT

News Office Massachusetts Institute of Technology — Room 5-111, 77 Massachusetts Avenue — Cambridge, Massachusetts 02139 — Telephone 617-253-2700

URGENT MEDIA ADVISORY

For Immediate Release May 1, 1989
MIT Contact: Eugene F. Mallove, Sc.D.
Chief Science Writer
617-253-2701

CAMBRIDGE, Mass., May 1 -- Professor Ronald R. Parker, Director of the MIT Plasma Fusion Center responded today to an article published this morning in the Boston Herald, an article that he says has seriously misquoted him and given a largely incorrect view of his discussions with the Boston Herald's reporter, Nick Tate.

Professor Parker issued this statement:

"The article erroneously characterizes remarks that I made regarding the cold fusion experiments done at the University of Utah. Specifically, I did not: (1) Deride the University of Utah experiments as "scientific schlock" or (2) Accuse Drs. Fleischmann and Pons of 'misrepresentation and maybe fraud'."

Today, Professor Parker's colleagues will present a paper (co-authored with him) at the meeting of the American Physical Society in Baltimore, Maryland, in which they suggest that data that Drs. Pons and Fleischmann claim support the observation of neutron emission in their experiments were misinterpreted by Pons and Fleischmann.

Based on their independent analysis, the MIT researchers say that if neutron emission occurred in the Pons and Fleischmann experiment that they reported in the *Journal of Electroanalytical Chemistry*, it would have been at a level far below that reported by the University of Utah group.

By June of 1989, the hot fusion community and the physics establishment were very satisfied that they had debunked cold fusion. Any of the growing numbers of positive reports could readily be dismissed by reporters and other, less involved scientists. After all, the plasma physicist authorities at MIT had spoken. In fact, so convinced were the PFC people that they had killed off cold fusion, they held a celebratory party—billed as a "Wake for Cold Fusion" on June 26, 1989. The humorous poster for the party notes: "Brought to you by the Center for Contrived Fantasies—Black Armbands Optional."

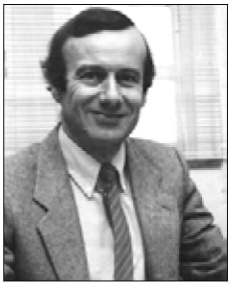
What is most interesting about this anything-but-funny mockery by the PFC is that at the time the party was held, the data for the Phase-II calorimetry experiments had not yet been analyzed! It was not until mid-July 1989 that the calorimetry data were put in anything like final published form. No formal conclusion had been set into print. How do we know this? Simple. In the course of my investigations into cold fusion, I would of course regularly ask PFC team members for their latest impressions, data, etc. So I was given many, many documents that piled up on my desk, not all being closely examined when received. But as I was completing *Fire from Ice* in the spring of 1991, questions about the PFC calorimetry had been brought up by my cold fusion colleague, electrochemist Dr. Vesco Noninski. Was the methodology and analysis of the PFC Phase-II calorimetry reported in the paper published by the PFC in the *Journal of Fusion Energy* sound? Noninski had many doubts and so did I. We approached a team member for clarification and got no satisfaction —just continued brush-off. I then looked through my stacks of papers from the PFC and found to my complete astonishment (and dismay) the two draft reports on the Phase-II calorimetry. One was dated July 10, 1989 and the other July 13, 1989, a clearly more complete version—the version that was actually published in both a formal PFC report and the *Journal of Fusion Energy*. Only a week after this MIT PFC analysis solidified, PFC Director Parker occupied himself dispensing "humorous" cold fusion mugs that were obtained "wholesale" in Utah (*see Exhibit F*, p. 90)!

Electrochemist Dr. Vesco C. Noninski questioned MIT PFC's analysis of its calorimetry data. *Photo by E.Mallove*

On June 7, 1991 I resigned from the MIT News Office, to protest the outrageous behavior of the PFC and others at MIT against cold fusion. Among other disgraceful happenings, an article of mine on cold fusion that had been approved for publication by the then editor of MIT *Technology Review*, was canceled after being trashed by MIT Physics Department Professor Herman Feshbach. Feshbach told me over the phone when I inquired, "I have fifty years of experience in nuclear physics and I know what is possible and what is impossible." He also told me that he did not want to see any more evidence for cold fusion, which I offered to show him, because, "It's all junk!"

MIT Prof. Herman Feshbach *MIT News Office*

Hours before my formal resignation, the PFC was having another of its "celebrations" for the death of cold fusion. Dr. Frank Close was speaking at a seminar there, billed "An Exposé of Cold Fusion," in which he lashed at Pons and Fleischmann for their alleged fudging of gamma ray curves. He had nothing of significance to say about the P&F calorimetry, consistent with this appalling high-energy physicist mind-set that "knew everything that could and could not happen" among nuclei. After Close was finished, Dr. Petrasso as master of ceremonies, very reluctantly gave me some time to comment. ("Just *one* minute, Gene!") I showed the July 10-July 13 curve shifting with overhead transparencies and suggested



Dr. Frank Close
Princeton University Press

Dr. Richard Petrasso
Photo by E. Mallove

New Energy Times Archive

sarcastically to Close that he should consider covering this important documentary finding in the next edition of his book (Heaven forfend that there should be another!). It was as though I were talking to a wall. This was not deemed important. After all, hadn't he just "proved" that cold fusion was dead?

PFC Director Parker then stated that this was the first time he had seen the data I had flashed on the screen—it probably was. Then Parker made the astounding assertion that "you can put those curves anywhere you wish." He publicly stated that the data from the MIT PFC was "worthless." (See *Exhibit K*, p. 92). Many weeks later, after I had revealed the PFC story to the world, Parker reverted to *defending* the conclusions of the calorimetry data—in an informal press release put out by the MIT News Office (See *Exhibit T*, p. 107). It must take many years of training to maintain such mutually contradictory opinions with a straight face—on national television and in written documents.

Let me be clear: There was likely no grand "conspiracy" to suppress a positive finding for excess heat in the MIT PFC-Phase-II calorimetry, it's just that the mind-set of the MIT hot fusioneers and Chemistry Department people allowed lower echelon persons to monkey with the data. He or she could not possibly bring anything to his superiors—Ronald Parker and then MIT Chemistry Dept. Head Mark Wrighton—that looked remotely positive for excess heat. This would have opened up the cold fusion story again in the summer of 1989, this time with MIT coming in with some encouraging news. So, the data was "fudged." I can think of another F-word—beyond "fudging"—that applies. It is closer to the truth. Ronald Parker likes to bandy it about in interviews with newspaper reporters. This groundless, manipulated and fabricated data has subsequently been cited over and over again by the U.S. Patent Office to reject cold fusion patent applications. It was even used, in part, ultimately to kill the Pons and Fleischmann patent itself, which happened in the Fall of 1997. Other MIT-trained cold fusion inventors have also had their patent applications attacked with this unscientific travesty from MIT.

Perhaps the most remarkable aspect of the MIT PFC experiment was that after I publicly challenged it, the objective of the experiment was *redefined* by its defenders! Thus, it is quite literally true that the experiment published in the *Journal of Fusion Energy* and the MIT PFC technical report is *by definition* fraudulent—*if only because the ground rules for comparing the heavy water and ordinary water experimental outputs were subsequently changed and are not as stated in the article.* These ground rules went from the obvious implication that can be taken from the lack of difference between the published curves to the statement that the MIT PFC team were looking for "fast turn on" of 79 mW excess heat and didn't find it! See NIH physicist Dr. Charles McCutchen's letters to the MIT Administration about this key point—*Exhibits Z-4, Z-8*, and *Z-11*, p. 114-118. Dr. Mitchell Swartz has produced a remarkable, clear analysis of the data produced by the MIT PFC—including all of the various inconsistent versions of the data and their interpretation). The work speaks for itself. Interested readers may request the original color-graphic paper which is included in a paperback book from JET Technology, P.O. Box 81135, Wellesley Hills, MA 02481. *Please see ad on page 100 for further information on ordering this book.*

Dr. Mitchell R. Swartz of JET Technology, Inc. lectured on cold fusion calorimetry, January 20, 1996 at Cambridge Marriott Hotel. Meeting sponsored by *Infinite Energy* Magazine.
Photo by E. Mallove

- Swartz, Dr. Mitchell R., "Re-Examination of a Key Cold Fusion Experiment: 'Phase-II' Calorimetry by the MIT Plasma Fusion Center," *Fusion Facts*, August 1992, pp. 27-40.
- Swartz, Dr. Mitchell R., "A Method to Improve Algorithms Used to Detect Steady State Excess Enthalpy,"*Proceedings: Fourth International Conference on Cold Fusion* (December 6-9, 1993, Lahaina, Maui, Hawaii), and in Transactions of *Fusion Technology*, Vol.26, December 1994, pp. 369-372.
- Swartz, Dr. Mitchell R., "Some Lessons from Optical Examination of the PFC Phase-II Calorimetric Curves, *Proceedings: Fourth International Conference on Cold Fusion* (December 6-9, 1993, Lahaina, Maui, Hawaii).

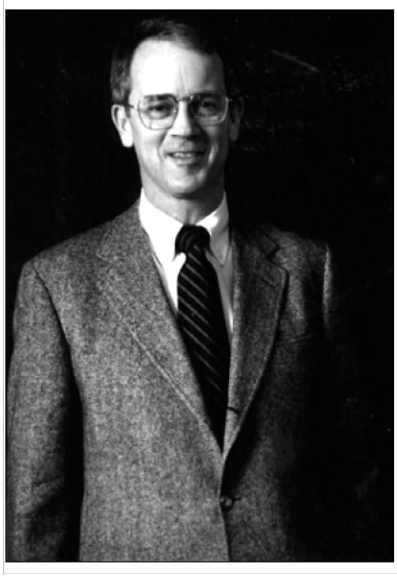
The Sham MIT "Inquiry"

I am very glad that Dr. Swartz undertook the task of this essential analysis, because certainly he was more capable than I in this kind of detailed examination of points that appeared and disappeared in various versions put out by the PFC. He did it himself after I turned over to him the materials that I had discovered. I was so revolted by the handling of this matter by the MIT Administration, that I really could not stand to wallow in the falsehoods coming out of the MIT PFC. My feeling was: "Let them stew in their own self-created problems. The world will eventually understand what they did." It will.

After my formal complaint to MIT President Charles Vest in August 1991 (See *Exhibit R*, p. 102), in which I asked for an appropriate investigation of scientific misconduct in the data handling and in the planting of a false press story by Parker in 1989, the whole matter was, in effect, swept under the rug by Vest after an utterly insufficient examination of the technical issue by Professor Professor Philip Morrison, who was a friend of MIT PFC report co-author, Dr. Petrasso.

Morrison's down-playing of the issues involved was a great disappointment, but not surprising for someone who to this day does not comprehend the significance of the research results in the cold fusion field. A symptom of this: To my knowledge, Prof. Morrison—at least as of early 1999—has never reviewed in his wide ranging columns *any* cold fusion books—either positive or negative. In one of his notes to President Vest (*Exhibit V*), Morrison stated that cold fusion findings "would at most open some way to build a new battery, possibly a fuel cell." This kind of ill-informed remark should be beneath the author of *The Ring of Truth!*

Concerning the ethical issues of Parker's dealings with the press and the MIT News Office, President Vest



MIT President Charles M. Vest, who continues to ignore cold fusion research. He excused the unethical behavior of MIT PFC staff against cold fusion. He is on a Federal panel that has advised the Clinton Administration to increase funding for hot fusion—a benefit for the MIT PFC (see Exhibits R through Z-11).
MIT Photo by Edward McCluney

New Energy Times Archive

stated that his legal counsel advised him no action was necessary. It was a shameful, sham "inquiry," not a thorough investigation, as the subsequent portion of this report and the various Exhibits show. I complained vigorously to President Vest that the inquiry was totally inadequate. In fact, the people who should should have been under investigation were allowed to continue to handle the data and write a subsequent "Technical Appendix" that made further excuses for data mishandling. As Dr. Swartz has shown, the data was, indeed, altered yet again during the "investigation"! For now, I hope that Dr. Swartz's analysis, and my own assessments and exchanges with President Vest, will be examined carefully by all who still have an open mind about the historical development of the cold fusion controversy.

My conclusions about the inappropriate data manipulation at the MIT PFC are my own and my *opinions about the implications of this data mishandling are to be considered distinct from Dr. Swartz's.* My assessments of the MIT calorimetry and data handling appear in my Letter of Resignation (*Exhibit L*, p. 94), my formal request for an investigation of scientific misconduct (*Exhibit R*, p. 102), and other exchanges with President Vest that form the exhibits to this report. But let me quote Dr. Swartz's summary conclusions from his fourteen-page technical paper:

From: Dr. Mitchell R. Swartz's, "Re-Examination of a Key Cold Fusion Experiment: 'Phase-II' Calorimetry by the MIT Plasma Fusion Center," *Fusion Facts*, August 1992, pp. 27-40.

The light water curve was published by the PFC essentially intact after the first baseline shift, whereas the heavy water curve was shifted a second time. The cells were matched,[12] and solvent loss would be expected to be greater for H_2O.

The Phase-II methodology is flawed because it masks a constant [steady-state] excess heat. Furthermore this paradigm fails to use either the true baseline drift, and may avoid the first 15% of the D_2O curve in Types 3, 3_B, 4, and 5 curves.

What constitutes "data reduction" is sometimes but not always open to scientific debate. The application of a low pass filter to an electrical signal or the cutting in half of a hologram properly constitute "data reduction," but the asymmetric shifting of one curve of a paired set is probably not. The removal of the entire steady state signal is also not classical "data reduction."

In the May 1992 Appendix, the PFC retroactively claims its "systematic errors now total 100 to 400 milliwatts, implying an insensitivity of >30 kilojoules.

Much current skepticism of the cold fusion phenomenon was created by the PFC paper's reporting "failure-to-reproduce."[12] as opposed to its later claimed "to insensitive-to-confirm" experiments[17]. Because it may be the single most widely quoted work used by critics of cold fusion to dismiss the phenomenon, the paper should have clarified all "data" points and the methodology used. Apparent curve proliferation, volatile points, asymmetric curve shifts, combined with an impaired methodology have needlessly degraded the sensitivity, and believability of the Phase II calorimetry experiment.

12. D. Albagli, R. Ballinger, V. Cammarata, X. Chen, R.M. Crooks, C. Fiore, M.P.J. Gaudreau, I. Hwang, C.K. Li, P. Linsay, S.C. Luckhardt, R.R. Parker, R.D. Petrasso, M.O. Schloh, K.W. Wensel, M.S. Wrighton, "Measurement and Analysis of Neutron and Gamma-Ray Emission Rates, other Fusion Products, and the Power in Electrochemical Cells Having Pd Cathodes," *Journal of Fusion Energy*, **9**, 133, 1990.

17. S.C. Luckhardt, "Technical Appendix to D. Albagli *et al., J. Fusion Energy,* 1990, Calorimetry Error Analysis," MIT Report PFC/RR-92-7, (May 1992).

Present MIT students as well as alumni should investigate this most unfortunate episode for themselves, and take action—for the well-being of MIT. There is no doubt in my mind that the MIT PFC calorimetry was mishandled and fraudulently misrepresented. Dr. Swartz's paper, using proper analysis that could have been performed by the MIT PFC, determined that "the average power by this method is 62 milliwatts (±34 milliwatts)." As Dr. Swartz states, this is "qualitatively similar to the value expected for a 'successful' experiment." Furthermore, Dr. Swartz credits in his references and conclusions my August 1991 complaint to President Vest (see *Exhibit R*, p. 102) that a "20% discrepancy in heater power, used to heat *the same volume of fluid*, has been suggested as corroborating evidence that the heavy water cell produced excess heat."

At the very least it was scientifically and morally required that the MIT PFC group repeat its experiments, rather than having them cited year after year against cold fusion, when they should have been retracted or corrected, per the suggestion of physicist Dr. Charles McCutchen—see *Exhibit Z-11*, p. 118. To cover up a sorry episode may have been comfortable for the MIT administration in an era in which cold fusion had not yet achieved general acceptance (thanks in no small way to some on the MIT staff), but that era will pass. An age of enlightenment is coming that will make the tokamak hot fusion program at MIT a footnote to history. The era of safe, clean, and abundant energy from water—non-chemical energy from hydrogen—will drown the deceivers from MIT to Princeton. (If anyone has any doubt about this emerging commercial reality, they should consult one of the energy-from-water corporations that was influenced by the announcement of Fleischmann and Pons—see BlackLight Power Corp. [www.blacklightpower.com]—pages 36-39 in this issue. No doubt many bright MIT graduates will be employed there.) No one can say that we did not warn MIT officials of the consequences if this important matter was allowed to be mishandled at MIT the way it was and continues to be.

Other Issues

The preceding is the basic story of what went on at MIT in 1989-1992. Much of this could have been avoided if President Vest had had an open-door policy toward appropriate scientific dissent. On April 12, 1991, I had sent a letter to President Vest (see *Exhibit I*, p. 90), at a time when I was feeling optimistic about what could be accomplished. I had hoped that the new MIT President, who had replaced the outgoing Dr. Paul E. Gray, would take action on its important message. I recommended that a study group be formed to re-examine what had been learned about cold fusion since 1989. Should I have been surprised at not receiving a response? Not when President Vest had chosen Chemistry Department head Mark Wrighton, to be Provost. Examine Wrighton's brusk and totally inappropriate response to Dr. Noninski (*Exhibit H*, p. 90). Wrighton's "let me make this perfectly clear I have no comment" letter is not a response that a scientist with integrity would have written.

After the events of 1991-1992, there followed many hard years of struggle, working with other engineers and scientists in cold fusion research, and trying to correct false impressions about cold fusion investigations that were being made by journalists and government officials. The launching of *Infinite Energy* magazine in 1995 (and its short-lived precursor, *Cold Fusion* magazine, 1994) was, in part, a response to the egregious distortions about cold fusion that were initiated by members of the MIT PFC.

Fire from Ice was well received by many reviewers, but its message was largely drowned out by an onslaught of scurrilous anti-cold fusion books, the first one by Frank Close, *Too Hot to Handle* (1991). Dr. Richard Petrasso of the MIT PFC had aided Close's

New Energy Times Archive

work. He was in complete agreement with Close's opinions; witness his comment published on the front page of the Sunday *New York Times*, March 17, 1991, which was essentially a laudatory review of the book by Close. Recall Dr. Petrasso's words: "I was convinced for a while it was absolute fraud. Now I've softened. They [Pons and Fleischmann] probably believed in what they were doing. But how they represented it was a dear violation of how science should be done." A case of the pot calling the kettle black, I'd say, in light of the technical publication to which Petrasso (and fifteen others) has signed his name. Nothing much has changed for Dr. Petrasso. In 1997 he was quoted by writer Bennett Daviss: "The ongoing reports of excess heat and nuclear by-products catch people's attention about as much as the occasional UFO report. I have better things to do with my time." (in *TWA Ambassador* article, September 1997, see reprint in IE No. 17). He and Professor Parker continue to spend *your* money on hot fusion.

There were other negative books, one by DoE's John Huizenga (*Cold Fusion: The Scientific Fiasco of the Century*, 1992), and another by science journalist Gary Taubes (*Bad Science: The Short Life and Weird Times of Cold Fusion*, 1993). Taubes became a Knight Science Journalism Fellow at MIT for a year, a nominal honor for him, if not a disgrace for MIT. The MIT News Office, to my knowledge, never published one word about the existence of *Fire from Ice*, nor the fact that *Fire from Ice* was nominated in 1991 for the Pulitzer Prize by John Wiley & Sons as one of only two of its books so nominated that year. Professors at MIT routinely bombard the News Office with requests that their every major or minor award be acknowledged in *Tech Talk*. Virtually all such requests are granted.

So goes PR at MIT—ever protective of the MIT Administration and its deficiencies—whether in flaps over an MIT student being killed by an alcohol overdose at an MIT fraternity after warnings were ignored by President Vest (See *Boston Globe*, October 1, 1997, p.1 "Students Warned MIT on Drinking—Complaints Began in 1992), or the very serious matter of data fudging and misrepresentation by MIT hot fusion scientists. The MIT Administration clearly was not happy by the spate of publicity that my resignation from the News Office generated. It acted accordingly.

The MIT PFC continues to receive Federal funding for its lucrative hot fusion projects—over $250 million since 1989. One of the ways that MIT helps to insure the continued flow of such funding is by having President Vest sit on the various Federal panels that make recommendations to the Administration and the the Department of Energy. Now that former Physics Dept. Head Professor Ernest Moniz is a Deputy U.S. Secretary of Energy, MIT's ability to bring influence to bear for hot fusion will be even stronger.

In a 1995 issue of the *Journal of Fusion Energy* we find "The U.S. Program of Fusion Energy Research and Development: Report of the Fusion Review Panel of the President's Council of Advisor's on Science and Technology (PCAST)," (Vol. 14, No. 2, 1995, pp. 213-250). One of the nine co-authors is none other than Charles M. Vest. The report's summary states, in part: "Funding for fusion energy R&D by the Federal government is an important investment in the development of an attractive and possibly essential new energy source for this country and the world in the middle of the next century and beyond. . . The private sector can not and will not bear much of the funding burden for fusion at this time because the development costs are too high and the potential economic returns too distant. But funding fusion is a bargain for society as a whole." That's their opinion, not ours. This is not even the opinion about hot fusion of many technologists who have nothing to do with cold fusion.

The report states, ". . .we believe there is a strong case for the funding levels for fusion currently proposed by the U.S. Department of Energy (DoE)—increasing from $366 million in FY1996 to about $860 million in FY2002 and averaging $645 million between FY1995 and FY2005." It goes on to acknowledge that "Although the program just described is reasonable and desirable, it does not appear to be realistic in the current climate of budgetary constraints. . ." So the report asks for less, in the tradition of grabbing for whatever the bureaucracy thinks it can get: ". . .to preserve what we believe to be the most indispensable elements of the U.S. fusion effort and associated international collaboration." The panel recommended about $320 million/year and continues further fantasy thinking about committing Federal $billions in continued support for ITER (International Thermonuclear Experimental Reactor). Fortunately, the U.S. Congress withdrew support from ITER in late 1998.

And, wonder of wonders, the 1995 report speaks directly about the need to continue to support MIT's Alcator C-Mod tokamak reactor. I suppose that in the general run of how Federal funding of science is promoted by numerous interest groups, this apparent conflict of interest—an MIT President recommending that MIT receive further funding for its hot fusion reactor— is not unusual. However, such advising by MIT's Vest is very unseemly, when seen in the context of the cut-off of all DoE funding for cold fusion, resulting from the 1989 negative report and from the MIT PFC experiment on which that report was based. Furthermore, as this history has made clear, President Vest played no small role in the whitewashing of this 1989 misconduct.

In another report, this one directly to President Clinton on November 4, 1997, "Report to the President on Federal Energy Research and Development for the Challenges of the Twenty-First Century," the Energy Research and Development Panel of PCAST, which includes Dr. Vest, we find the general recommendation spelled out in the cover letter to President Clinton: "The report recommends an increase, over a five-year period, of $1 billion in the Department of Energy's annual budget for applied energy technology R&D. The largest share of such an increase would go to R&D in energy efficiency and renewable energy technologies, but nuclear fusion and fission would also receive increases. The composition of the R&D supported on advanced fossil-fuel technologies would change in favor of long-term opportunities, including fuel cells and carbon sequestration technologies, but the overall spending level for fossil fuel technologies would stay roughly constant in real terms." In table "ES.1" we find the fusion wish list after 1998 in "millions of as-spent dollars": 1997—$232 (actual); 1998—$225 (request); 1999—$250; 2000—$270; 2001—$290; 2002—$320; 2003—$328.

The report states that the request for fusion is the "third largest increase" of the various energy items. It calls the funding ". . .easily justified as the sort of investment government should be making in a high-risk but potentially very-high yield energy option for society, in which the size and time horizon of the program essentially rule out private funding." Well, virtually all of the scientists working in cold fusion in 1999 think that cold fusion is, indeed, "a very-high yield energy option" for society. Private industry has invested in it in a limited way, and more will come. If it were not for the Federally paid scientists—in hot fusion and high energy physics—who assaulted cold fusion with lies and deceptions—there would likely be even more private money now flowing into cold fusion. One thing is certain: no private company in its right mind will spend any significant money on tokamak hot fusion, as practiced at MIT and elsewhere.

What it boils down to is this: By studying the history MIT and cold fusion, one learns that paradigm-paralyzed and unethical scientists have the motive and means to wreck massive damage against an emerging science and technology, especially when an aging and well-financed

New Energy Times Archive

program is threatened. An MIT President who has access to the highest power levels of the Federal government should not be contributing to the distortion of government spending by feathering MIT's nest and ignoring facts. MIT alumni/ae, students, staff, and President Charles M. Vest need to consider this—E. Mallove

Letter by Julian Schwinger

Re: Eugene Mallove's *Fire from Ice*

Letter of February 5, 1991 from physics Nobel Laureate Julian Schwinger (Nobel Prize for physics in 1965, shared with Sin-Itiro Tomanaga and Richard P. Feynman "for their fundamental work in quantum electrodynamics, with deep-ploughing consequences for the physics of elementary particles"). This handwritten letter was sent to John Wiley & Sons, Inc., concerning the manuscript of Eugene Mallove's book, *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor*, which would soon be published in May 1991. [Note: Italics and square brackets have been added by E. Mallove]

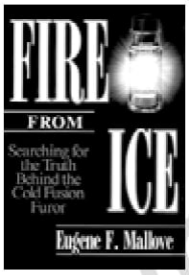
Dear Judith McCarthy [John Wiley & Sons]:

Thank you very much for sending me Mallove's typescript. For almost two years, I have been muttering: "Someone has to write a book about this!" "This"is the bizarre story of cold fusion—its bizarre science, and its bizarre human behavior. The author of that book would need some familiarity with the relevant physics (atomic and nuclear), chemistry (electrolysis, at least), and should have had first-hand experience of some of the events and their participants. But, most of all, he must have a balanced view that incorporates an understanding of what the "scientific method" really means.

I have just finished reading every word of 470 pages of typescript. (In modest proof thereof, I offer two 'Typos. . .etc.) *I enjoyed it very much. Eugene Mallove, in my book, is the right one to write about "the truth behind cold fusion."*

I have enclosed two recent articles of mine, one delivered the day before December 7 [1990], in Tokyo, the other a short supplement that has been submitted to a Japanese journal. Please send them on to E.M. (beyond MIT, I am unaware of his address) for his possible interest and, at least, amusement.

I should also like to add, vis-a-vis his recognition of the absurdity of the Editorial note on p. 435, that its promise—"duty to give him the opportunity to explain his ideas and present his case. . ." was a lie. Only the short introductory note, Part 1, was published. When Part 2 and the much more substantive Part B were submitted, they received the usual vituperative reviews and were rejected; they have never been published.



Incidentally, the other paper of mine cited on p. 551, *Cold Fusion: A Hypothesis*, which was published after more than a year's delay, went first to PRL [*Physical Review Letters*]. Although I anticipated rejection, I was staggered by the heights (depths?) to which the calumny reached. My only recourse was to resign from the American Physical Society, (APS).

You ask for ". . .a few words . . ." Perhaps they can be found above. If not, how about: *Eugene Mallove has produced a sorely needed, accessible overview of the cold fusion muddle. By sweeping away stubbornly held preconceptions, he bares the truth implicit in a provocative variety of experiments.*

Yours, Julian Schwinger

P.S. I am grateful for E.M. for quoting A.C.D. [Arthur Conan Doyle] on p. 216. I have long been conscious of that bit of sherlock Holmes wisdom, but could not recall the particular story in which it appears. J.S.

Other Comments on *Fire from Ice*

by MIT-Affiliated People (from the book jacket)

"Mallove brings dramatically to life the human side of this important scientific controversy, which has tapped the emotions of its scientific participants in a way usually typical only of major scientific revolutions. *Fire from Ice* is highly recommended reading for anyone who is interested in the nature of scientific controversy and scientific change. I frankly could not put the book down once I had started it."

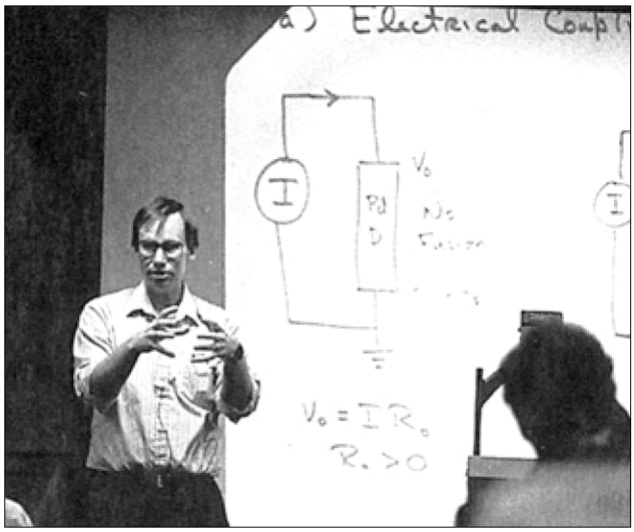
—Dr. Frank Sulloway, former MacArthur Fellow, science historian, MIT Program in Science., Technology, and Society

"*Fire from Ice* is a masterpiece of scientific documentation. Progress in deciphering the cold fusion effect is now stalemated by an establishment pressure for conformity. An authoritative book needed to be written, and it had to come from someone with roots in both the science and journalism communities; there are very few people in the world as qualified as Eugene Mallove is to write it and give the story the meticulous attention it required."

—Dr. Henry Kolm, co-founder of MIT's Francis Bitter National Magnet Laboratory

Dr. Petrasso, Prof. Hagelstein, and Prof. Fleischmann at First International Conference on Cold Fusion, 1990, Salt Lake City.

Photo by E. Mallove

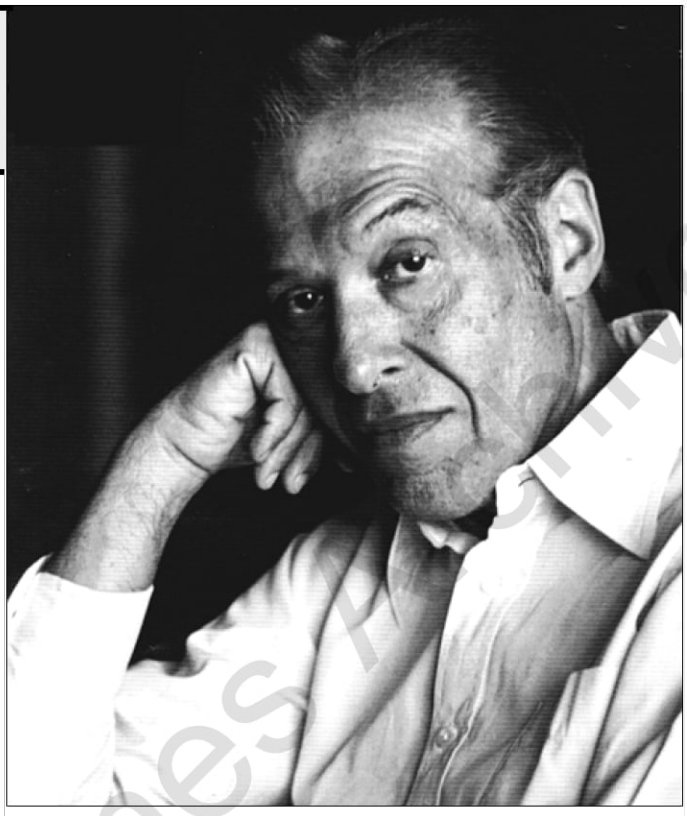


Prof. Peter Hagelstein lecturing on cold fusion theory at MIT April,1989.

MIT Photo

New Energy Times Archive

A lecture by cold fusion theorist Nobel Laureate Julian Schwinger, November 11, 1991, at MIT celebrating the 60th birthday of Professor Kenneth Johnson—a former student



In Nobel Laureate Julian Schwinger's eloquent talk at MIT, he compared the possible theoretical foundation of cold fusion with that of the much more accepted but equally mysterious phenomenon, sonoluminescence. Julian Schwinger had resigned from the American Physical Society (APS) to protest its censorship of his theoretical work on cold fusion from APS publications. It was an honor for me to have become a good friend of Schwinger's due to my involvement with cold fusion. His praise for my book, Fire from Ice, *was a very great honor (see page 80). Unfortunately, Schwinger's 1991 message at MIT was not absorbed by the assembled MIT physicists.—EFM*

A Progress Report: Energy Transfer in Cold Fusion and Sonoluminescence

by Julian Schwinger, University of California

Birthday celebrations are inevitably somewhat nostalgic. Appropriately, then, I found the cover title for this lecture in my own distant past. I first came to Berkeley on the day that World War II began. Not long after, Robert Oppenheimer gave a lecture—perhaps on cosmic ray physics—which he called "A Progress Report," in the sense, he explained, that time had elapsed. A similar expression of modesty is in order here. I have no great discoveries to announce; only feelings, hypotheses, and programs. As Mort Sahl once proclaimed:

The future lies ahead.

I am sure that my first topic, cold fusion, has caused many eyebrows to levitate. Cold fusion? Isn't all that nonsense dead and buried? How can anyone be so insane as to talk about this totally discredited subject?

Well, to the extent that sanity implies conformity with the mores of a society—didn't the Soviets clap their egregious dissidents into insane asylums?—sanity, I submit, is not a canon of science. Indeed, isn't it a goal of physics, specifically, to push at the frontiers of accepted theory through suitably designed experiments, not only to extend those frontiers, but, more importantly, to find fundamental flaws that demand the introduction of new and revolutionary physics?

—sanity, I submit, is not a canon of science. Indeed, isn't it a goal of physics, specifically, to push at the frontiers of accepted theory through suitably designed experiments, not only to extend those frontiers, but, more importantly, to find fundamental flaws that demand the introduction of new and revolutionary physics?

The seemingly bizarre behavior of some key players in the cold fusion melodrama has managed to obscure a fundamental challenge that this episode presents. Whether or not the reality of cold fusion has been demonstrated experimentally, one must ask if any conceivable mechanism now exists, or might be devised, whereby nuclear energy could be extracted by manipulations at the atomic level.

One is mindful of the high temperature superconductivity story. Despite the assurances of theorists that superconductivity could not exist much above absolute zero, that barrier was broken experimentally. Although it took time to get reproducible results, the reality of the phenomenon is completely established, despite the absence (to my knowledge) of any accepted theory.

High temperature superconductivity is an atomic process. Cold fusion is that too, but also involves the much shorter space and time scales of nuclear physics. It should therefore be much more difficult to control this phenomenon by manipulations at the atomic, perhaps better said: at the chemical, level. More difficult, but not necessarily impossible.

Despite my earlier qualification of the established reality of cold fusion, one cannot ignore the evidence accumulated in many laboratories—of excess heat production, of tritium production—all of which is characterized by irreproducibility and by uncontrollable emission in bursts. But, from what has just been said, that kind of behavior is expected; it is not a basis for rejecting the reality of the phenomena.

This brings me to study the validity of the case against cold fusion, as seen by a hot fusioneer—henceforth known as HF—who rejects the possibility that new physics is involved.

In the hot fusion of two deuterons—the D-D reaction—the formation of a triton (^3H) and a proton proceeds at about the same rate as that for the creation of ^3He and a neutron. But, given the claims of tritium production in cold fusion experiments, neutrons at the expected intensities are conspicuously absent, although low levels of neutrons, appearing in bursts,

New Energy Times Archive

have been observed. To HF the conclusion is obvious: No neutrons—no tritium—no cold fusion. Moreover, the two cited reactions are the only important ones in hot fusion. So: No neutrons—no cold fusion—no excess heat.

Very soon after March 23, 1989—which one might well call D-day—the idea was advanced that excess heat is produced by the formation of ^{4}He in the ground state. To this HF responds that the suggested reaction is weak, and no one has detected the γ-rays of roughly 20 MeV that should accompany the formation of ^{4}He.

Then came the suggestion that excess heat might result from the HD, rather than the DD, reaction. Heavy water (D_2O) always has some small contamination of light water (H_2O). The fusion of a proton and a deuteron produces ^{3}He. To which HF responds that no γ-ray of roughly 5 MeV, which should accompany this reaction, has been observed.

With heat production and tritium production allocated to the HD and DD reactions, respectively, how can one understand the suppression of neutron production? It may be that two fusing deuterons populate, not the quite remote ground state, but rather the first excited state of ^{4}He. That excited state decays into a triton and a proton. But, decay into ^{3}He and a neutron is energetically forbidden. Tritium—Yes. Neutrons—No. HF responds to this by pointing to the absence of the roughly 4 MeV γ-ray that should accompany the ^{4}He excited state.

Thus presented, the experimental aspects of HF's indictment of cold fusion come down to the non-existence of various γ-rays that the tenets of hot fusion require. What rebuttal can one give to these charges?

Well, consider the following bit of insanity:

The circumstances of cold fusion are not those of hot fusion.

In contrast with hot fusion, where energies are measured in substantial multiples of kilovolts, cold fusion deals with energies that are a fraction of a volt. The dominant electromagnetic mechanism for hot fusion is electric dipole radiation, in which the parity of the particle system reverses.

Now, at the very low energy of cold fusion, two deuterons, for example, which carry even intrinsic parity, have very little chance of fusing in other than the orbital state of zero relative angular momentum—of even orbital parity. Thus, an excited state of ^{4}He is formed that has even parity. Possibly it radiates down to the first excited state, or the ground state of ^{4}He. But both of the latter states also have even parity. With no parity change, electric dipole radiation is forbidden. There are, of course, other mechanisms that might intervene, albeit much more weakly—electric quadrupole radiation, magnetic dipole radiation, electron-positron pairs. But, much more important is the impetus this result gives to considering the following additional bit of insanity:

The excess energy liberated in cold fusion is not significantly transferred by radiation.

If not radiation, what? HF, with his focus on near-vacuum conditions, would have no answer. But cold fusion does not occur in vacuum—it appears in a palladium lattice within which deuterium has been packed to form a sub-lattice. Which leads to the next bit of insanity:

The excess energy of cold fusion is transferred to the lattice.

This is the moment to introduce HF's theoretical ace in the hole. In hot fusion work it is taken for granted that the fusion reaction rate is the product of two factors: the barrier penetration probability that stems from the Coulomb repulsion of like charges; and the intrinsic reaction rate that refers mainly to the nuclear forces. At the very low energy of cold fusion, the penetrability of the Coulomb barrier is so overwhelmingly small that nothing could possibly happen.

How does one respond to that? By sharpening the initial insight:

The circumstances of cold fusion are not those of hot fusion.

At the very low energy of cold fusion, one is dealing essentially with a single wave function, which does not permit the factorization that HF takes for granted. The effect of Coulomb repulsion cannot be completely separated from the effect of the strongly attractive nuclear forces. This is a new ball game.

All very well, but can one be a little more specific about the new mechanisms that might produce cold fusion?

If, as I hypothesized, the lattice is a basic part of that mechanism, some knowledge of the palladium lattice, loaded with deuterium, is needed. That knowledge exists for light loading, but, as far as I am aware, not for heavy loading. There is, however, a theoretical suggestion that, for sufficiently heavy loading, a pair of new equilibrium sites, for hydrogen or deuterium ions, comes into being within each lattice cell. The equilibrium separation of such a pair is significantly smaller than any other ionic spacing in a cell.

It would seem that, to take advantage of those special sites, a close approach to saturation loading is required. (Indeed, that is so if a steady output is to occur.) But, the loading of deuterium into the palladium lattice does not proceed with perfect spatial uniformity. There are fluctuations. It may happen that a microscopically large—if macroscopically small—region of the lattice attains a state of such uniformity that it can function collectively in absorbing the excess nuclear energy released in an act of fusion.

And that energy might initiate a chain reaction as the vibrations of the excited ions bring them into closer proximity. This burst of energy will continue until the increasing number of irregularities in the lattice produce a shut-down. The start-up of another burst is an independent affair. It is just such intermittency—of random turnings on and off—that characterize those experiments that lead one to claim the reality of cold fusion.

Now we come to barrier penetration, or rather, what replaces it. HF accepts a causal order in which the release of energy—at the nuclear level—into the ambient environment, follows the penetration of the Coulomb barrier. The response to that carefully crafted statement is surely: Of course! What else? Well, how about this major bit of insanity?

Other causal orders and mechanisms exist.

Unlike the near-vacuum of HF, the ambient environment of cold fusion is the lattice, which is a dynamical system capable of storing and exchanging energy.

The initial stage of one new mechanism can be described as an energy fluctuation, within the uniform lattice segment, that takes energy at the nuclear level from a dd or a pd pair and transfers it to the rest of the lattice, leaving the pair in a virtual state of negative energy. This description becomes more explicit in the language of phonons. The non-linearities associated with large displacements constitute a source of the phonons of the small amplitude, linear regime. Intense phonon emission can leave the particle pair in a virtual negative energy state.

To illustrate the final stage of this mechanism, consider the pd example where there is a stable bound state: ^{3}He. If the energy of the virtual state nearly coincides with that of ^{3}He a resonant situation exists, leading to amplification, rather than Coulomb barrier suppression. Between the two extremes of causal order there are, of course, a myriad of intermediate energy transfer mechanisms, so that the mechanism, as a whole is devoid of causal order.

I note here the interesting possibility that the ^{3}He produced in the pd fusion reaction may undergo a secondary reaction with another deuteron of the lattice, yielding ^{5}Li. The latter is unsta-

New Energy Times Archive

ble against disintegration into a proton and ^{4}He. Thus, protons are not consumed in the overall reaction, which generates ^{4}He.

The suggestion that nuclear energy could be transferred to an atomic lattice is usually dismissed (contemptuously, I might add) because of the great disparity between atomic and nuclear energy scales; of the order 10^7, say. It is, therefore, of great psychological importance that one can point to a phenomenon in which the transfer of energy between different scales involves--and here I quote—"a focusing or amplification of about eleven orders of magnitude."

It all began with the sea trials, in 1894, of the destroyer HMS Daring. The onset, at high speeds, of severe propeller vibrations led to the suggestion that bubbles were forming and collapsing—the phenomenon of cavitation. Some twenty-three years later, during World War I, Lord Rayleigh, no less, was brought in to study the problem. He agreed that cavitation, with its accompanying production of pressure, turbulence, and heat, was the culprit. And, of course, he devised a theory of cavitation. But, there, he seems to have fallen into the same error as did Isaac Newton who, in his theory of sound assumed isothermal conditions. As Laplace pointed out in 1816, under circumstances of rapid change, adiabatic conditions are more appropriate.

During World War I, the growing need to detect enemy submarines led to the development of what was then called (by the British, anyway) subaqueous sound-ranging. The consequent improvements in strong acoustic sources found no scientific applications until 1927. It was then discovered that, when a high intensity sound field produced cavitation in water, hydrogen peroxide was formed. Some five years later came a conjecture that, if cavitation could produce such large chemical energies, it might also generate visible light. This was confirmed in 1934, thereby initiating the subject of sonoluminescence (SL). I should, however, qualify the initial discovery as that of incoherent SL, for, as cavitation noise attests, bubbles are randomly and uncontrollably created and destroyed.

The first hint of coherent SL occurred in 1970 when SL was observed without accompanying cavitation noise. This indicates that circumstances exist in which bubbles are stable. But not until 1990 was it demonstrated that an SL stream of light could be produced by a single stable cavity.

Ordinarily, a cavity in a liquid is unstable. But it can be stabilized by the alternating cycles of compression and expansion that an acoustic field produces, provided the sonic amplitudes and frequencies are properly chosen. The study of coherent SL, now under way at UCLA under the direction of Professor Seth Putterman, has yielded some remarkable results.

What, to the naked eye, appears as a steady, dim blue light, a photomultiplier reveals to be a clock-like sequence of pulses in step with the sonic period, which is of the order of 10^{-4} seconds. Each pulse contains about 10^5 photons, which are emitted in less than 50 pico seconds, that is, in about 10^{-11} seconds.

When I first heard about coherent SL, some months ago, my immediate reaction was: This is the dynamical Casimir effect. The static Casimir effect, as usually presented, is a short-range non-classical attractive force between parallel conducting plates situated in a vacuum. Related effects appear for other geometries, and for dielectric bodies instead of conductors.

A bubble in water is a hole in a dielectric medium. Under the influence of an oscillating acoustical field, the bubble expands and contracts, with an intrinsic time scale that may be considerably shorter than that of the acoustical field. The accelerated motions of the dielectrical material create a time-dependent—dynamical—electromagnetic field, which is a source of radiation. Owing to the large fractional change in bubble dimensions that may occur, the relation between field and source could be highly nonlinear, resulting in substantial frequency amplification.

The mechanisms that have been suggested for cold fusion and sonoluminescence are quite different. But they both depend significantly on nonlinear effects. Put in that light, the failures of naive intuition are understandable.

So ends my Progress Report.

Julian Schwinger's cold fusion work has been published in non-APS journals, including the *Proceedings of the National Academy of Sciences*. We proudly reprinted his "Cold Fusion: A Brief History of Mine," in Issue No.1 of *Infinite Energy*, 1995.

For a few years, the "cold fusion underground" at MIT held a well-attended cold fusion symposium during the IAP (Independent Activities Period). Since 1996, this activity has moved off campus.—EFM

COLD FUSION
A Massachusetts Institute of Technology
IAP Program—Video-Lecture-Demonstration Program

January 21,1995, Saturday 9AM-5PM
Room 6-120, Physics Lecture Hall
First floor, main building of MIT.

TENTATIVE PROGRAM - Subject to Change

Start at 9:00 am sharp

* Dr. Eugene F. Mallove, MIT'69, Organizer -—Introduction, outline, and overview of latest results (30-45 min)
* Dr. Peter Graneau (Video tape of water plasma explosions) "Anomalous Forces in Water Plasma Explosions" (45-60 min)
* J. Patterson's U.S. Patent and Technology—video tape and lecture by staff of Clean Energy Technology, Dallas, TX (30 min)
* James Griggs—The Hydrosonic Pump (video and lecture) (45 min)
* Coffee Break
* Ray Conley, MIT -- Results of Light Water Excess Heat Experi–ments (20min)
* Fred Jaeger, ENECO (Patents and Commercialization) (10 min)
* Recent results of experiments at E-Quest Sciences—Helium and Excess Heat (10 min)
* Lunch Break of 20-25 minutes, refreshments to be served outside 6-120
* Professor Peter L. Hagelstein, MIT "Neutron Transfer Reactions"—Progress in theory (45 min)
* Professor Keith Johnson, MIT, Progress in Theory of Excess Heat and Progress in Producing "Cold Fusion: The Movie" (45 min)
* Professor Vesco Noninski, Fitchburg State College "Nuclear measurements—new understandings" (20 min)
* Bertil Werjefelt, PolyTech(USA) (45 min) "'Magnetic Energy': Experiments, Commercial Prospects, and Theory"
* Video Tape from Japan, Fuji Television (8 minutes)—"Magnetic Energy"
* Time allotted for late-arriving additions in cold fusion and enhanced energy
* CBC Cold Fusion Program, "Too Close to the Sun" (50 min)
* Evening Break at 5:00 p.m. for dinner and possibly resume for 7:00-8:30

General Discussion of Business and Social Issues—Possible Panel Discussion. Refreshments and organizing costs contributed by ENECO, a company committed to commercialization of cold fusion and enhanced energy technologies.

The full tapes of the program and a written record summarizing the meeting will also be available through Dr. Gene Mallove, Bow, NH.

[Tapes may still be ordered from Cold Fusion Technology—see page 124]

New Energy Times Archive

EXHIBIT A

While the MIT PFC-Chemistry Department team was going through the early stages of its motions to "debunk" the work of Drs. Fleischmann and Pons, one of the team members, Professor Ronald Ballinger, was sent to Washington to testify before Congress. The MIT hot fusion people wanted to minimize the chance that Congress would divert any hot fusion funding to the investigation of cold fusion. In his testimony, Ballinger audaciously claimed that the MIT calorimetry methods were more sophisticated than those of Fleischmann and Pons—a great irony in view of later serious questions about the MIT PFC work. While this Congressional blocking action was carried out, the plan to launch a PR assault against cold fusion was moving forward. Only two days later, Professors Ballinger and Ronald R. Parker would give a secret interview with Boston Herald *reporter Nick Tate (see Exhibit B), the story that would mark the beginning of accusations of fraud against the Utah electrochemists.—Eugene Mallove (EFM).*

Comments on "Cold Fusion"

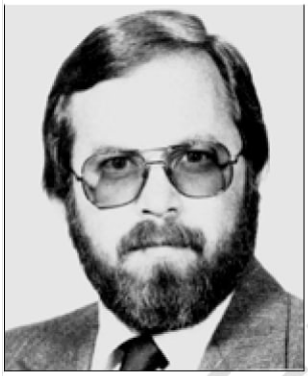
Testimony presented to the Committee on
Science, Space, and Technology
U.S. House of Representatives
Washington, D. C.

by Professor Ronald G. Ballinger, Department of Nuclear Engineering,
Department of Materials Science and Engineering,
Massachusetts Institute of Technology, Cambridge, Massachusetts

April 26, 1989

Mr. Chairman, Members of the Committee:

I am Ronald Ballinger, a faculty member of the Departments of Nuclear Engineering and Materials Science and Engineering at the Massachusetts Institute of Technology. I am very grateful for your invitation to convey my views related to the recent reports of the achievement of "cold fusion."



Professor Ronald G. Ballinger
MIT Photo

I am a member of an interdisciplinary team at MIT that is involved in an attempt to reproduce the reported "Cold Fusion" results of Professors Pons and Fleischmann of the University of Utah. The team's principals include Dr. Ronald R. Parker, Director of MIT's Plasma Fusion Center; Professor Mark S. Wrighton, Head of the Chemistry Department; and myself. (A complete list of team members and areas of expertise is included). The team is composed of experts in the fields of physical metallurgy, electrochemistry, plasma physics, instrumentation, and radiation detection. The team has been involved in attempts to reproduce the results, reported by Professors Pons and Fleischmann since shortly after their results were released to the press and for publication in the *Journal of Electroanalytical Chemistry*.

As I am sure that you and the members of this committee are aware, any breakthrough in the area of energy production that has the potential to supply current and future energy needs in a non-polluting manner must be given serious attention. Quite apart from its impact on basic science, the results recently reported by Professors Pons and Fleischmann, should they prove to be correct, represent such a breakthrough. The basic nature of their results have been described and discussed by earlier testimony before this committee. Basically, the team at the University of Utah has reported the fusion of deuterium atoms in a palladium matrix at room temperature.

As evidence that "cold fusion" has taken place, the production of excess heat and neutron radiation has been reported. The reported magnitude of both of these is such that their presence could be verified by other investigators.

Much more modest results have been reported by a team of investigators at Brigham Young University. We feel that it is important to distinguish between the BYU results, which are of scientific interest but of limited or no practical significance and those of the University of Utah which, should they prove correct, have major implications for future energy production.

Since the reports of these results, a number of teams worldwide have been attempting to reproduce these results. To my knowledge, with the possible exception of the Stanford results and results from Europe and the USSR of which I have no personal knowledge, no team has been successful. As far as the results of attempts by the team at MIT are concerned, we have been thus far unable to scientifically verify any of these results. This is in spite of the fact that we are employing calorimetry and radiation detection methods of even greater sophistication and sensitivity than those of the University of Utah. Having said this, I can assure you that these negative results have not been the results of a lack of effort. The MIT team has been, as I am sure is the case with other teams, laboring around the clock. However, we and the other teams have been handicapped by a lack of enough scientific detail to guarantee that we are actually duplicating these experiments.

In the scientific community, the soundness of experimental or theoretical research results is evaluated through peer review and duplication. For results such as those reported, whose potential impact on the scientific community and the world are so great, this review process is absolutely essential. Unfortunately, for reasons that are not clear to me, this has not happened in this case—at least so far. The level of detail concerning the experimental procedures, conditions and results necessary for verification of the Pons and Fleischmann results have not been forthcoming. At the same time, almost daily articles in the press, often in conflict with the facts, have raised the public expectations, possibly for naught, that our energy problem has been "solved." We have heard the phrase "too cheap to meter" applied to other forms of electric energy production before. And so the scientific community has been left to attempt to reproduce and verify a potentially major scientific breakthrough while getting its experimental details from the *Wall Street Journal* and other news publications.

Experiments conducted in haste and based on insufficient detail coupled with premature release of results have often resulted in retractions and embarrassment on the part of the scientific community—caught in the heat of the moment. I guess we are all human.

New Energy Times Archive

The result of this unsatisfactory situation has been that a healthy skepticism and, in some cases, distrust of the reported results has developed. We at MIT share this skepticism.

At the risk of becoming too technical in my comments, I feel that I must be a bit more specific with regard to the source of this skepticism. As I mentioned earlier the major results, reported by the University of Utah group, are that there has been a generation of excess heat and the measurement of neutron radiation. By excess heat I mean that there has been a measurement of more energy produced than has been supplied to the system. From our standpoint, the key point of verification is the detection of neutron radiation. From an engineering point of view, however, the importance of excess heat production is critical. On these two critical points we have found that the results reported in the few available published documents from the University of Utah are inconclusive or unclear. For example, with respect to the detection of neutrons, critical products of the fusion reaction, the reported results are confusing. They either do not agree with or are not presented completely enough to show that they are consistent with what one would expect from the emission of neutrons from the deuterium fusion reaction. Specifically, the γ-ray spectrum shown in the Fleischmann/Pons paper and attributed to neutron emission does not exhibit a shape and intensity that demonstrates the increase reported in the number of detected neutrons above normal background. Further, the reported rate of neutron emission and level of tritium production are consistent with natural background. The results have nevertheless been reported as "significant." Those inconsistencies can only be resolved by a full disclosure of the details of the experimental measurements for examination by the scientific community. Until such time as this occurs we feel that the data is insufficient to demonstrate the presence of neutrons.

As far as the issue of excess energy is concerned we are also faced with a confusing situation. While the presence of excess energy is documented in the *Journal of Analytical Electrochemistry* paper, the method by which this excess energy was determined is not clear. With metals, such as palladium, which act as hydrogen storage media and at the same time as catalysts for many chemical reactions, both situations which can result in discontinuous chemical energy releases, it is critical that a total energy balance over time be done. To us it is not clear that this has been the case. Until this issue is clarified we are unable to make a judgement concerning the excess energy issue.

In conclusion, I feel that it is safe to say that the scientific community is (1) excited about the possibility of a significant advance in the area of fusion energy research, (2) but is, at the same time, skeptical of results that have not been verified to this point and (3) is very frustrated at the methods by which the discovery has been handled both in the scientific and non-scientific community. Thank you.

PROFESSOR RONALD GEORGE BALLINGER

Professor Ballinger is an Associate Professor at the Massachusetts Institute of Technology with a joint appointment in the Departments of Nuclear Engineering and Materials Science & Engineering. Professor Ballinger's areas of specialization are as follows: (1) Environmental effects on material behavior, (2) Physical metallurgical and electrochemical aspects of environmentally assisted cracking in aqueous systems, (3) Stress corrosion cracking and hydrogen embrittlement in Light Water Reactor systems, (4) The effect of radiation on aqueous chemistry and stress corrosion cracking, (5) Experimental fracture mechanics techniques and analytical methodology, and (6) Materials development for cryogenic applications. Professor Ballinger is the author of several papers in the above areas and is a member of several professional societies including the National Association of Corrosion Engineers, The American Society for Metals, The Electrochemical Society, The American Nuclear Society, and the American Society for Testing and Materials. Professor Ballinger is a member of the International Cyclic Crack Growth Review Group and the International Cooperative Working Group in Irradiation Assisted Stress Corrosion Cracking.

MIT Cold Fusion Group

Plasma Fusion Center

Professor Ronald R. Parker
Director, Plasma Fusion Center
Plasma Physics/Fusion Research

Dr. Xing Chen
Postdoctoral Associate
Radiation Detection

Dr. Catherine Fiore
Research Scientist
Radiation Detection

Dr. Marcel Gaudreau
Research Engineer
Fusion Engineering

Dr. David Gwinn
Research Engineer
Instrumentation/Design

Dr. Paul S. Linsay
Principal Research Scientist
Radiation Physics

Dr. Stanley Luckhardt
Principal Research Scientist
Plasma Physics

Dr. Richard Petrasso
Research Scientist
X-and γ-ray Spectroscopist

Mr. Kevin Wenzel
Graduate Student
Radiation Detection

Dept. of Nuclear Engineering and Dept. of Materials Science and Engineering

Professor Ronald G. Ballinger
Associate Professor
Physical Metallurgy and Electrochemistry

Dr. Il Soon Hwang
Research Scientist
Physical Metallurgy/Electrochemistry

Dr. Alan Turnbull
Visiting Scientist (National Physical Laboratory, UK)
Electrochemistry/Surface Science

Martin Morra
Graduate Student
Physical Metallurgy

Mr. Frank Wong
Graduate Student
Mechanics/Instrumentation

Department of Chemistry

Professor Mark Wrighton
Head, Department of Chemistry
Chemistry/Electrochemistry

Dr. Richard Crooks
Postdoctoral Associate
Electrochemistry

Mr. Vincenzo Cammarata
Graduate Student
Chemistry/Electrochemistry

Mr. Martin Schloh
Graduate Student
Chemistry/Electrochemistry

Mr. David Albagli
Graduate Student
Chemistry/Electrochemistry

"Words to Eat"

MIT Professor Ronald George Ballinger may hold the all-time record for making a foolish statement against cold fusion. He wrote in 1991: "It would not matter to me if a thousand other investigations were to subsequently perform experiments that see excess heat. These results may all be correct, but it would be an insult to these investigators to connect them with Pons and Fleischmann."

These words of "wisdom" appeared in the *Gordon Institute News*, March/April 1991. Apart from their unrepentant mean spirit, they are internally inconsistent. If in his *hypothetical* the remarkable discovery of Fleischmann and Pons were to be validated, why would the scientists not be due praise? Is Ballinger's sense of righteous indignation about Fleischmann and Pons so pronounced that he could not grant them credit—*ever*? One would think that scientific ethics alone would mandate that these "thousand other investigations" should be tied directly to those who inspired them!

Ballinger wrote in the same venue: "Putting the 'Cold Fusion' issue on the same page with Wien, Rayleigh-Jeans, Davison-Germer, Einstein, and Planck is analogous to comparing a Dick Tracy comic book story with the Bible." The facts about this moralizing hypocrite, Prof. Ballinger, are even more amazing when one learns that Ballinger subsequently personally sought funding support from Dr. Thomas O. Passell at the Electric Power Research Institute (EPRI) to carry out materials science projects related to cold fusion!

New Energy Times Archive

Exhibit B: Partial transcript of tape of interview (Friday, April 28, 1989) by Nick Tate of the *Boston Herald* with Professor Ronald R. Parker and Associate Professor Ronald G. Ballinger

This tape was released to the public, according to a *Boston Herald* story on May 2, 1989. The contents of this tape clearly contradict the MIT Press Release of May 1, 1989, which was issued by the MIT News Office on behalf of Professor Parker.

Prof. Ballinger

Prof. Parker

Nick Tate, then of the *Boston Herald*

From this "smoking gun" interview, it is clear that the story in the Boston Herald, *May 1, 1989, was an eminently fair reflection of the interview with Professors Parker and Ballinger. Virtually the full April 28, 1989 interview with Parker and Ballinger is transcribed from the audio tape released by the* Boston Herald. *It is fascinating to hear Parker telling a DoE Cold Fusion Panelist (Dr. Richard Garwin of IBM) that they think they have evidence of "fraud" by Fleischmann and Pons. Parker is seen coordinating with NBC reporter Bob Bazell the forthcoming "blast" against the Utah electrochemists.—EFM*

[***Editor's Note:** "xxxx" means inaudible portion of audio tape.*]

Parker: . . .accuse them of fraud, one could.

Tate: Can you—can you tell me what the uh—what exactly the significance of the 2.5 is? I mean, understand I've attempted—I'm not a scientist. I've attempted to read as much as I can understand.

Parker: I can give you a short synopsis of that.

Ballinger: Can we, uh—can I make—say something here about —? You're going to publish this right?

Tate: Yes.

Ballinger: You're not a scientific person, right?

Tate: That's correct.

Ballinger: What's the procedure about this? Can we see what you're going to print, before you're going to print it? Not to change anything, but to make sure you don't screw something up here.

Tate: In technical terms?

Ballinger: In technical terms.

Tate: Uhhh—

Ballinger: You know we're talking about serious business here and I have seen crap in newspapers that comes out, you know, that quotes the generation of isotopes which aren't—don't exist and all kinds of stuff like that. Nobody's going to change anything—

Parker: That's a good point. The reason I stopped talking to the *Globe*, for example, is that I felt that they were reporting irresponsibly.

Ballinger: They interviewed me but didn't (xxxx). . .

Parker: Yeah, and you know they were out there just leading the cheers instead of being objective.

Tate: Let me say this to you, that in general the process is—the policy of the paper is to turn down a story before having it proof read outside of the paper. But I understand what you're saying. I think that given I am not a scientific person, we could work something out.

Ballinger: There has to be a way because there's sort of a moral obligation here on our part to make sure that . . .

Parker: Let me—Yeah let me put it another way, I mean we're beginning to get a very short fuse on this whole issue, as you can tell, because for example these guys were down in Congress when Ron was down there on Wednesday asking for twenty-five million bucks. [***Editor's Note:** See Ballinger's Congressional testimony, page 84.*]

Ballinger: A hundred and twenty five.

Parker: Well, a hundred. . .

Tate: Was it $125 million?

Parker: Only a mere twenty-five from the government, right?

Ballinger: Twenty-five from the government, the rest from industry.

Parker: And, you know, it's one thing when they come out with something that's potentially interesting scientifically and so on and so forth. It's quite another thing when they're out there trying to fleece the public money to push something that, uh, has no credibility at this point. Now in a (xxxx) way what we're saying is we're ready to begin getting into the controversial issue.

Tate: I should explain to you. . .

Parker: We don't want to do that without trusting the source, Okay? In other words, you know I can't trust the *Globe*, I'd like to trust you. I can't trust you unless I know what you're going to turn out.

Tate: I guess it depends on what we talk about. What you're suggesting is that what I would like to do based on just a little information that I've heard is write a story that indicates you have serious questions and concerns about what Pons and Fleischmann are saying. . .

Parker: We can go beyond the concerns and questions to say that what they have reported is not true. That's a much stronger statement.

Tate: And potentially what you're suggesting is that—to bring some money into the university.

Parker: I shouldn't say that, I should say that that's your conjecture, not mine. Okay, the fact that they're down there asking for $125 million you can draw your own conclusions from that.

Ballinger: I would suggest that you probably have a tape of the entire hearing.

Parker: Do you have one?

Tate: I don't have one, no.

Ballinger: Well, you should get one and you should look at it and spend the time, because then you'll understand what was going on down there. In terms of your background, it's a very important thing for you to look at. Even though you may be on

New Energy Times Archive

Parker: Do you have one?

Tate: I don't have one, no.

Ballinger: Well, you should get one and you should look at it and spend the time, because then you'll understand what was going on down there. In terms of your background, it's a very important thing for you to look at. Even though you may be on a deadline and it may be six hours long and all that stuff, you really need to see what was going on.

Tate: What was your impression of what was happening?

Ballinger: It was a fairly well-orchestrated attempt to, in my mind, short circuit in this case well-established and well-recognized review process for any kind of research much less this kind of research and get uh—diversion of funds to uh from the government from other projects presumably to the University of Utah. And, uh, they used uh you know. If you assume that what they are saying is correct, you may argue about the heavy-handedness of what their logic is of their methods, but for something that's not been proved to be correct, the fact that they used this (xxx) was going on plus the fact that (xxx). . .

Ira Magaziner
He later played a role in the Clinton Administration on health policy. *Photo: Courtesy White House*

Ballinger: If you don't look at the tape, you should read Ira Magaziner's testimony. He is the consultant that they hired to uh . . .

Parker: Is he the one who went into this society in Rome?

Ballinger: He made a very, you know, a very pseudo-truth — that's the word I'd use. You start out with something which is fundamentally true, but everything is not so true after that. We are, in fact, getting killed by the Japanese. I mean we're great inventors, but we don't do a good job of bringing things to market. The Japanese are excellent at that and were getting beaten. We're getting our ass beaten, right? And that's the argument that he used, we should definitely—we should stuff all kinds of money in here, we should go on parallel paths, we should establish a center—an international center in Utah, naturally in Utah, because that's where the best scientists are. And we should get going on this right away. That makes sense, if you're trying to, if you have an established, verified product. You know, I mean I think I agree that we're getting killed by the Japanese and so therefore we should —there has to be a way to augment the way we do that kind of thing. But he started from a fundamental assumption which was not correct, and that is, we don't have anything that's proven, and moreover not only do we not have anything that is proven, but there's a lot of reason to believe that not only will it be disproven, but it will turn out that it's not correct.

Tate: Let me ask you, just back up a step. You're talking about—I presume you're talking about traditional scientific controls and traditional scientific methods that have not been observed in this particular situation.

Parker: This is sci— I'll give you a quote: This is scientific schlock, Okay.

Tate: Tell me specifically what they've done.

Parker: [Parker laughs].

Tate: That is that may. . .

Parker: I'll just tell you about the neutrons, Okay.. That's really important, Okay. They've taken some data. They didn't even take it themselves, they had people take it for them. They published it in their paper and they claimed that it showed the presence of neutrons from their experiment. The data is patently, has been patently falsely interpreted. Neutrons are not present at anywhere near the level their own data shows. They're not there. They've misinterpreted their results. They falsely interpreted their results. Whether they did this intentionally or not I don't know, but they did not present—interpret their results correctly. It's a key point in their paper.

Tate: Specifically what they're claiming, that it was neutrons they were creating. . .

Parker: That they were creating neutrons from their experiment. Their documentation unfortunately shows that not only was it falsely interpreted, but there were no neutrons at anywhere near the level they claimed. You can use the data in two ways, to show that they falsely interpreted it, but also that there weren't neutrons at the level they claimed.

Tate: So at best it's misinterpretation and at worst it's — as you were saying. . .

Parker: It's fraud.

Tate: Now do you know this from studying their research, from reviewing their information, or have you tried—and I presume you've, in addition, attempted to parallel what they've done?

Parker: We reproduce their results so we completely understand why they misinterpreted. Let me put it a different way, we don't see why they misinterpreted, we don't understand what they should have seen and didn't.

Tate: So you've reproduced their experiment?

Parker: We've simulated the neutrons. We've said, suppose there were no neutrons, what would it have looked like? And we find something quite different from what they claim.

Ballinger: We find what we should expect.

Tate: Would you care to speculate on their intent?

Parker: I think Ron made it perfectly clear that when you're asking for $125 million for the university, I mean I don't want to be tied into that quote, but I mean you have to draw the (xxxx). They were in Washington Wednesday asking for $125 million dollars.

Dr. Richard Garwin
Photo: IBM Corporation

[Editor's Note: At this point in the interview, Parker gets a phone call from Dr. Richard Garwin of IBM Corporation, one of the key people on the U.S. Department of Energy's cold fusion review panel. . .]

Parker (to Garwin): I just talked to Richard [Petrasso] who wrote the *Nature* piece. I don't know if you saw that? But he and I basically chuck it off, I mean you know I said his piece was the best thing written so far. And he told me he saw the original submission and it did have the line at 2.5 [MeV]. The original submission had the line at 2.5 so, you know that's, uh, the smoking gun with fingerprints, Okay, you don't even need (xxxx). Oh, gee, I don't want to quote him, but that's a good question, but the original submission to the journal had 2.5, just as the 2.5 in the equation, so you know now it transcends I think the question of whether they misinterpreted to the question of whether there was deliberate fraud. Okay, alright. . .(xxx). Well, all your detective work was correct, but now he has the smok-

New Energy Times Archive

ing gun with the fingerprints on it, right? [Laughs] Okay, right, see ya! Bye. . .

Parker: So I thought he would be good to talk to and he just volunteered. He'd seen the original submission to the journal. The line was at 2.5.

Prof. Huggins
Photo, Stanford University

Ballinger: That's what we suspected.

Parker: Right (xxxx). . .

Parker: This is where I'd say, uh, we just don't know, I mean —misinterpretation or was it malicious. . .

[Editor's Note: Now Parker remarks about Prof. Huggins' positive results in his Stanford University replication of P&F's experiment.]

Parker: Unfortunately I've seen that paper. I'd give it a B as a senior thesis . . .

[Parker and Ballinger laugh intensely about Fleischmann's implication about Japanese work.]

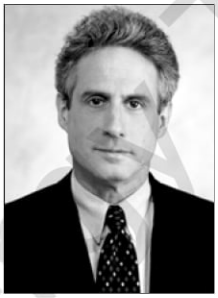
Parker: So, what are you going to do with this, uh, Nick? You know this is. . .what you're hearing is that we think it's a scam, right?

Tate: Why is it today that you think it's a scam?

Parker: We have been studying the evidence together very slowly and we want to have a paper out on this before we actually blast them. Monday we're putting a paper out on it. . .

Parker: It depends on what magnitude you want to break it.

Tate: Well, it seems to me that it's a very significant story for you to be saying...



Robert Bazell
Photo, NBC TV

Parker: It's the first time I've actually been (xxx) this strong. Up until now I've been hoping...

Tate: I mean everybody thinks you have been very skeptical, as have other teams (xxx) can reproduce it. . .

Parker: Open to the possibility. I think after five weeks we are basically getting to the point where we can no longer suspend the disbelief.

[Parker gets a phone call from science reporter Bob Bazell of NBC-TV]

Parker (to Bazell): Hello, Bob. Thanks for calling me back. Okay, appreciate it because uh (xxxx) we don't want them to have a chance to uh come up with any sort of (xxxx) Now I promise on Monday we'll have it out. I'll fax it to you. Okay, alright? I've got one in my office! Ha, ha. It's a local paper. No, we have not done anything as far as a press release. . .Uh, well maybe we can work something out. It depends on how big a story he wants to do. Well, if they didn't see neutrons. You know I just talked by the way to Richard Garwin and he confirmed that the first paper that Pons and Fleischmann submitted had the line at 2.5 MeV. Did you know that? Well, that's important because they moved it. And now the question is, is it fraud, or is it (xxxx)?

Parker: Well, that was Bazell, Bob Bazell — you know who he is, of NBC? — he's a little concerned about how you're going to handle it. He's concerned and I am too, because he's been very good to me as far as being confidential and respecting my views. He's at the (xxxx) right now where he wants to run something on this. And I feel like I'd like to, you know, I don't mind if it hits the streets the same day, but I think it would be. . .

Tate: That's fine.

Parker: I think if you'd respect that we can probably give you more. . .

Eugene Mallove

Tate: I would just ask that no other media outlets get this information beforehand. I think that's fair.

Parker: I was just thinking in my mind. I have a list of sort of good (xxxx). . .

Ballinger: *Technology Review*. . .

Parker: Yeah, they'll come out months from now. We'll have to give it to MIT actually, I mean Mallove.

Tate: I'm not real hot to scoop anybody with the story. It's a big story. I'd like to do that and respect your wishes. But if it comes out in another publication, a competitor or a daily publication . . .

Parker: It's not coming out in the *Globe*.

Tate: Okay.

Ballinger: And I don't answer phone calls unless they're from inside MIT. . .

Tate: Obviously, we're going to need to get into more of the technical aspects of it. Can you tell me some of those for that story in Monday's paper or would you prefer to handle that?

Parker: I'm going to have to leave in ten minutes anyway, so it's not going to be great. . . Let's see, how to handle it. We're going to get into trouble with Mallove, if we don't apprise him on Monday. But you could break the story on Monday.

Harold P. Furth
Princeton Plasma Physics Laboratory

Parker: [Parker on the phone to Harold Furth of Princeton Plasma Physics Lab]. . . We're also working with a guy called Wrighton, who is an electrochemist. . . Next week we're definitely going to hit them. . . So meanwhile, we're pretty much going to blast these guys on Monday — on the neutrons. . . Well you know, you can take that one on. I'm not going to get into the calorimetry. I think, having done the calorimetry for several weeks now, I understand much better about the problems, and I think I could speculate on what they did or didn't do. I certainly know enough to discount completely the Stanford experiment, only because they published enough details so I could see where they went wrong. Now in the case of Utah, they didn't publish details, so I can't say. . . All I'm going to focus on— I know the following facts. They published a peak initially at 2.5, they then moved it to 2.2 for the same data, alright? Now that could be either fraud or it could be just misinterpretation. I'm not going to comment on that. How-

"In one word, it's garbage."

MIT Professor of Physics Emeritus Martin Deutsch

May 6, 1989, characterizing cold fusion.

New Energy Times Archive

Exhibit C
MIT News Office Deceptive Press Release

I had been up into the wee hours of the night of April 30-May 1, 1989, sending a press release dictated to me over the telephone at my home in Bow, New Hampshire by Professor Parker. I telephoned it to UPI, Reuters, and the Associated Press, and it denied what Parker had said in the interview with the Boston Herald's *Nick Tate. When I arrived at the MIT News Office early that morning after a sleepless night, we hastily put together a printed form of the press release to handle the approaching storm. This is the text of the Press Release that was issued from the MIT News Office on May 1, 1989. On the day of my resignation from my MIT News Office position, June 7, 1991, I publically disavowed this Press Release—an unintended falsification of the truth in which I was used as a dupe in part of an orchestrated campaign against cold fusion (an image of this Press Release appears on page 76)—EFM*

MIT News Office PRESS RELEASE May 1, 1989
URGENT MEDIA ADVISORY

For Immediate Release May 1, 1989
MIT Contact: Eugene F. Mallove, Sc.D. Chief Science Writer

CAMBRIDGE, Mass., May 1—Professor Ronald R. Parker, Director of the MIT Plasma Fusion Center responded today to an article published this morning in the *Boston Herald*, an article that he says has seriously misquoted him and given a largely incorrect view of his discussions with the *Boston Herald's* reporter, Nick Tate.

Professor Parker issued this statement:

"The article erroneously characterizes remarks that I made regarding the cold fusion experiments done at the University of Utah. Specifically, I did not: (1) Deride the University of Utah experiments as "scientific schlock" or (2) Accuse Drs. Fleischmann and Pons of 'misrepresentation and maybe fraud'."

Today, Professor Parker's colleagues will present a paper (co-authored with him) at the meeting of the American Physical Society in Baltimore, Maryland, in which they suggest that data that Drs. Pons and Fleischmann claim support the observation of neutron emission in their experiments were misinterpreted by Pons and Fleischmann.

Based on their independent analysis, the MIT researchers say that if neutron emission occurred in the Pons and Fleischmann experiment that they reported in the *Journal of Electroanalytical Chemistry*, it would have been at a level far below that reported by the University of Utah group.

Exhibit D — *Boston Globe* Letter to MIT President Paul Gray —April 17, 1989

There is convincing evidence (see Exhibit B, p. 86) that Prof. Parker had made a deliberate attempt to exclude the "cheerleading" Boston Globe *from getting access to the MIT PFC in the hectic early days of the cold fusion uproar. Frustrated* Globe *science writer Richard Saltus wrote an extraordinary letter on April 17, 1989 to then MIT President Paul E. Gray. This was* before *the* Herald's *bombshell story of May 1, 1989 broke!—EFM*

The Boston Globe, Boston Massachusetts 02107
Telephone 617-929-2000

Paul E. Gray, President, Massachusetts Institute of Technology

Dear Dr. Gray:

We in the *Sci-Tech* section have always regarded MIT as a rich and crucial source of ideas and information. The News Office does an invaluable job in bringing stories to our attention, and the faculty and staff generally are very helpful when we call on their expertise. In addition, your visit to the *Globe* left a strong impression, and created renewed interest in subjects like science and math education and global environmental issues.

It has been disturbing, therefore, to have encountered such a lack of cooperation—a selective one, it appears—from the leadership of the Plasma Fusion program during our reporting of the claimed breakthrough at the University of Utah. We felt our readers would want to know how this scientific controversy has affected a premier fusion research center—and one in the *Globe's* city.

However, repeated attempts by myself and another reporter to talk to individuals at the Plasma Fusion Center have met with little success. In the first weeks of the story, Dr. Ronald Parker did make himself available on a few occasions, but for the past week or more has failed to return phone calls.

I also was utterly rebuffed in an attempt, which I had cleared with the News Office, to visit the fusion center briefly— entirely at Dr. Parker's convenience—so that I could convey something of the activity there during this highly unusual time. I called several times, dropped by once, and left telephone numbers where I could be reached, saying I'd be glad to talk with anyone who had a free moment. The secretary promised to let me know what could be arranged. However, no one ever responded. I appreciate the enormous demands on Dr. Parker's time. Yet, he has found time for other publications, including the *Washington Post* —whose reporter toured the facility, took photographs and interviewed several researchers and the *New York Times*, which quoted Dr. Parker as recently as last Sunday.

Whether this selective access reflects caprice or some bias against the *Globe* is hard to tell. In any case, it is regrettable that we have had to give up on MIT and turn to institutions like Princeton, where, although I am sure they are no less busy, researchers have been more helpful.

Sincerely,
Richard Saltus, Science writer

cc: Dr. Ronald Parker, Plasma Fusion Center

Exhibit E — MIT President Paul Gray's Letter to the *Boston Globe* May 1, 1989

MIT President Gray, apparently unaware of Parker's true dealings, was himself duped into writing what he honestly thought was a valid response to the Globe's *Saltus.—EFM*

OFFICE OF THE PRESIDENT

Richard Saltus, Science Writer
The Boston Globe

Dear Mr. Saltus:

I write in response to your letter of April 17, which laments the recent inaccessibility of Professor Ronald Parker. I have looked into this and find that Professor Parker has been deluged by requests for information about cold fusion and is unable to respond to all of them. He has tried to be as helpful as possible, consistent with his belief that judgment should be reserved until the scientific facts are clarified. That cautious stance has led him to discourage all media visits to the Plasma Fusion Center, although his efforts have not always been successful.

I have been assured that there was no discrimination against the *Boston Globe* and that, to the contrary, Professor Parker spoke five or six times with your colleague, Mr. David Chandler.

I regret that you feel ill used during these recent events, but I am satisfied that they are merely a consequence of the extraordinary circumstances attendant to the claim of cold fusion. We certainly hope that our good relations with the *Boston Globe*, and with the *Sci-Tech* section in particular will continue.

Sincerely yours,

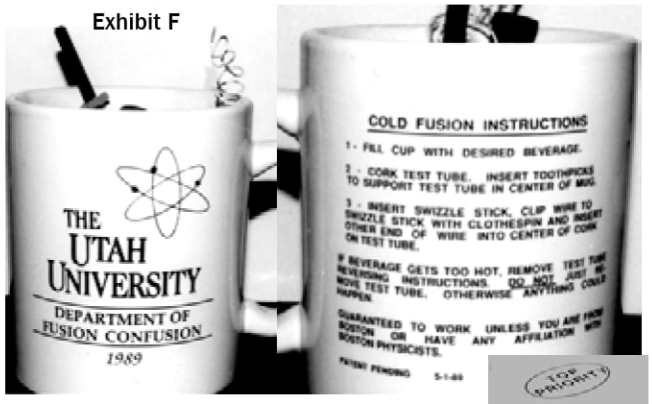
Paul E. Gray

PEG/mmd Signed in his absence
cc: Kenneth D. Campbell, Ronald R. Parker

New Energy Times Archive

Photos, Infinite Energy archives

Exhibit F



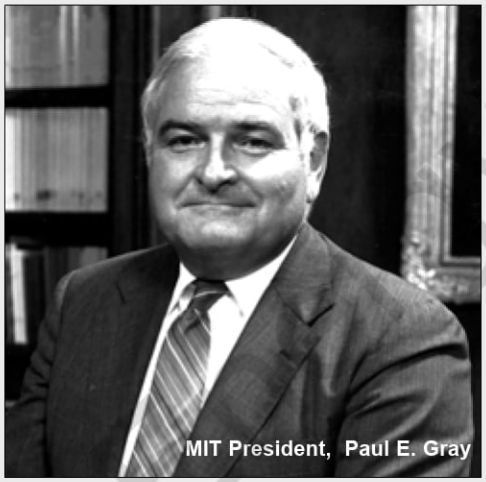
Plasma Fusion Center
Massachusetts Institute of Technology

To: Terri Priest
From: Ron Parker
Subj: Cold fusion Mug
Date: July 18, 1989

Thanks for your thoughtful procurement of the "cold fusion" mug. I really enjoyed it and will keep it with my "stamp out scientific schlock" tee-shirt and other cold fusion memorabilia. We have ordered two dozen (at quantity discount) for souvenirs to members of the MIT Cold Fusion Group. When they arrive, I'll send you one in case you know of someone else who would enjoy it.

Thanks again!

Exhibit G - MIT President Paul Gray's 1990 Remarks on Cold vs. Hot Fusion


MIT President, Paul E. Gray

This public statement clearly shows how badly the MIT PFC had duped the rest of the MIT community.—EFM

"If ever there was, in the media's eye, a silver bullet, 'cold fusion' certainly fit the bill. According to the first news release, it was 'simple, safe, and easy to implement.' Unfortunately, all the media attention surrounding the controversy over the veracity of the cold fusion experiments has overshadowed the quality work that has gone into 'hot' plasma fusion research over the last forty-five years. Here the potential energy payoff is so great and the scientific and political motivation so strong that a very large and productive research effort is already in place."

Energy and The Environment in the 21st Century (Proceedings of a Conference held at MIT March 26-28, 1990), MIT Press, 1991, p. 119-136, in "Energy Technology: Problems and Solutions," by Paul E. Gray, Jefferson W. Tester, and David O. Wood.

Exhibit H
Prof. Mark Wrighton's Letter to Dr. V. C. Noninski
October 10, 1990

This brusque letter from Prof. Wrighton, offering no scientific discussion, is an insult, yet so symptomatic of how the MIT Administration went about its anti-cold fusion work.—EFM

MASSACHUSETTS INSTITUTE OF TECHNOLOGY
DEPARTMENT OF CHEMISTRY

MARK S. WRIGHTON
DEPARTMENT HEAD AND
CIBA-GEIGY PROFESSOR OF CHEMISTRY

Dr. V.C. Noninski
New York, NY

Dear Dr. Noninski:

Unfortunately, I have not had time to review your various pieces of correspondence with me concerning our work directed toward establishing the validity of claims concerning cold fusion. Let me be perfectly clear with you: we have obtained no evidence whatsoever to verify the original claims by Pons and Fleischmann concerning cold fusion. I believe that we have indicated the nature of the errors involved in the calorimetry that we have done and do not believe that there is experimentally significant evolution of "excess heat."

Sincerely yours,
Mark S. Wrighton

MSW:jvs
cc: Dr. S. Luckhardt

Exhibit I
Eugene Mallove's Letter to MIT President Charles Vest
April 12, 1991

My urgent letter to President Vest, copied to President Gray, went unanswered. Should I have been surprised? Not when President Vest had chosen Chemistry Department head Professor Mark Wrighton as Provost. Wrighton was a co-leader of the 1989 MIT PFC cold fusion experiments and a signer of the 1989 negative DoE cold fusion report. If President Vest had given him my letter to review, Wrighton would probably have dumped it in his circular file.—EFM

Eugene F. Mallove, Sc.D., Chief Science Writer
MIT News Office, Room 5-111
Lecturer in Science Journalism, Department of Humanities
Massachusetts Institute of Technology

President Charles M. Vest
Massachusetts Institute of Technology

Dear Dr. Vest:

I am reminded of wonders wrought by science and technology on this day, the 30th anniversary of the first flight into space by a human being, Yuri Gagarin, and also the 10th anniversary of the flight of our space shuttle. I recall my feelings of awe—as a child and later as a young engineer, that human beings could accomplish these wondrous things. It seems that on the frontiers of science and technology, when dedicated men and women give their energies to a task, they can achieve wonders.

We are now facing, I believe, a new wonder in science. It is one, to be sure, that seems to be having an exceedingly difficult birth. I speak of what some people consider to be preposterous and "pathological" science, but others whom I believe have probed deeper into the matter, consider to be no longer deniable: that unusual nuclear reactions of incompletely understood character have been produced in metal lattice systems. Of course I am speaking of the controversial "cold fusion" phenomena. As

New Energy Times Archive

Nobel Laureate Julian Schwinger said in March 1990, "It is no longer possible lightly to dismiss the reality of cold fusion."

After long and careful study of this controversy in both its scientific, media, and political dimensions, I am personally convinced at greater than a 99% confidence level that cold fusion is real—both the nuclear emanations that have been reported and the excess enthalpy that seems to emerge from various experiments. The erratic nature of the phenomena —the lack of reproducibility "on demand"—has clearly been the central obstacle to acceptance in the scientific community, but extraneous "political" and programmatic factors have also played a role. It would seem, however, that reproducibility—no doubt a function of certain critical atomic structure and composition factors in the test systems—is getting to be less and less a problem.

Two unusual documents that have come to my attention are only the most recent in a cascade of information that is now emerging in the field. Physicist David Worledge of the Electric Power Research Institute (EPRI), who has just returned from a trip to the Soviet Union, supplied me with the astounding report of the "Workshop on Nuclear Fusion Reactions in Condensed Media," which was held at a world-class high-energy physics center under the sponsorship of the USSR Academy of Sciences, among other prestigious scientific organizations. This is extraordinary because this heretofore unknown but suspected level of effort on cold fusion in the Soviet Union gives an independent check on some of the nuclear effects work in the U.S. (I regret to say that in the present atmosphere of hostility to cold fusion in the U.S., such a conference would now be unthinkable at places like Brookhaven National Laboratory and Fermilab.) Noteworthy is the claimed increasing levels of reproducibility in the experiments, which incidentally, is also happening in the U..S—*e.g.* at Los Alamos National Laboratory and at SRI International in Palo Alto, which has carried out reproducible excess energy production in electrochemical cells.

Dr. Worledge of EPRI

The other paper comes from my colleague, Dr. M. Srinivasan, Head of the Neutron Physics Division at the Bhabha Atomic Research Center, BARC, in India. This is his recent excellent summary of the experimental evidence for cold fusion, which BARC has played a major role in supplying. Read it and perhaps be amazed, as I have been. I was already aware of most of the results that he cites, but he assembles it so nicely.

Dr. M. Srinivasan of BARC

As for experimental work here at MIT in this exciting new field, I regret to tell you that it does not exist. After the initial brief but intense period of experimental assessment in the spring of 1989 by an interdisciplinary team drawn from the Plasma Fusion Center and from the Chemistry Department, led by PFC Director Professor Ronald R. Parker and then Chemistry Department head, Professor Mark S. Wrighton, to my knowledge, nothing further has been done along experimental lines. It is notable, however, that researchers in several departments at MIT have continued a strong interest in the field.

An atmosphere of hostility, analogous to the editorial position on cold fusion of a certain well-known scientific journal [*Nature*], is prevalent. I do not feel that MIT's best interests are served any longer by unwarranted ignoring of the mounting experimental evidence for cold fusion. It seems to me essential that members of the MIT community reassess the experimental findings that have come and are coming from both domestic and foreign laboratories. To do any less would be, it seems to me, an abdication of scientific responsibility, not to mention a possible longer range injury to the reputation of MIT. It is even possible that the international competitiveness position of the U.S. might be at stake, something we here have given much attention to. There is strong evidence, for example the enclosed Matsushita Corporation patent application, that Japanese laboratories are devoting their considerable talents to this field. (I believe it probable that a major Japanese Corporation may be funding the work of Drs. Fleischmann and Pons now in France.) [*Editor's Note: That corporation turned out to be IMRA, an affiliate of Toyota Corp.—EFM*]

Basically, I think the train is leaving the station, and MIT is not on it. This deeply troubles, saddens, even embarrasses me —as an alumnus who cares deeply about MIT and its image. May I suggest that you assemble very soon and publicly a panel of MIT scientists and engineers to consider and evaluate the status of research on "nuclear reactions in deuterium infused metals." (There is no need to call it the politically charged, "cold fusion," even though that may well be what it is.) I can imagine the composition of such a panel, who would hear from researchers both within and from outside MIT—including from foreign countries. Obviously, MIT's thoughtful skeptics (*e.g.* Dr. Richard Petrasso) as well as proponents of these phenomena (*e.g* Professor Peter Hagelstein) should be aboard. As a chairperson, I would offer the names of three outstanding scientists, who could guide deliberations in a fair manner: Professors Philip Morrison, Jerome Friedman, or Henry Kendall. Because of my knowledge of the field through being a conduit of information, I would be honored to assist any such panel in its deliberations.

I have sent a copy of this letter to your predecessor, Professor Paul Gray, with whom I have discussed cold fusion earlier, in the days when the controversy arose. My deep appreciation to you for carefully considering this suggestion. I look forward to discussing the idea further with you, if you feel that it has merit, and of course I hope you will.

Sincerely, Eugene F. Mallove

Exhibit J
Eugene Mallove's Letter to Dr. Stanley Luckhardt
April 29, 1991

My written request to Dr. Luckhardt for clarification and other data was rebuffed.—EFM

Eugene F. Mallove, Sc.D., Chief Science Writer, MIT News Office, Room 5-111, Lecturer in Science Journalism, Department of Humanities

Dr. Stanley C. Luckhardt
Room 36-293

Dear Stan:

Glad that you were able to come to Dr. Fred Mayer's [cold fusion] seminar last week and ask some good questions. It's nice to have an alternate theory to compare with Peter's [Hagelstein's] ideas.

I have been meaning to submit a short note to the *Journal of Fusion Energy*, a comment of sorts about the MIT experiments in the spring of 1989 and where they fit into the big picture. I would mainly be addressing the calorimetry issue and in that regard would want to refer to both your perspective and to that of Dr. Noninski. I realize that there are two pieces of information that it would be helpful, though not essential, for me to have: (A) The precision and assumed accuracy of each of the measuring devices (current, voltage, and temperature) and (B) The plot of the heater power versus time for the light water comparison cell run that corresponds to the D_2O heater power plot presented in the PFC report.

Thanks in advance for your help, and I look forward to sharing with you some of my ideas, once I get them on paper.

Sincerely, Eugene F. Mallove

New Energy Times Archive

Exhibit K

Question and Answer Session for Frank Close's talk at MIT Plasma Fusion Center ("Too Hot to Handle: An Exposé on Cold Fusion"), Friday June 7, 1991. (Final interchange, in which the PFC Director Ronald Parker was introduced by Richard Petrasso)

Transcription by Eugene F. Mallove. Bold type sections are of particular interest (bold added by E. Mallove).

SEMINAR SEMINAR SEMINAR

Plasma Fusion Center

Dr. Frank Close

Rutherford Appleton Laboratories
Oak Ridge National Laboratories

Too Hot To Handle: An Exposé on Cold Fusion

Friday, June 7, 11 am

ROOM NW16-213

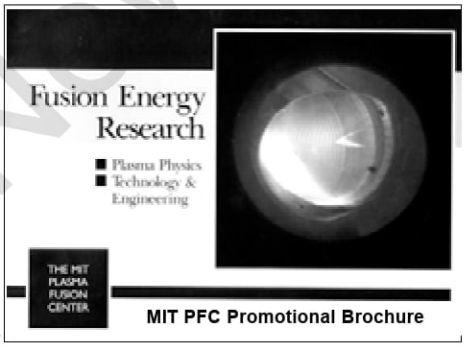
Refreshments will be served

Massachusetts Institute of Technology

Matters came to a head on June 7, 1991, when—unknown to me until the very few days before it occurred—a lecture by a strong critic of cold fusion, Dr. Frank Close of the UK, was scheduled for a Friday seminar at the Plasma Fusion Center. The posters for the talk proclaimed it to be "An Exposé on Cold Fusion"—and indeed, it was just that— a slanderous attack of Fleischmann and Pons! It turned out to be a climactic event in my career and in the history of cold fusion.—EFM

Parker: Looking at the Pons and Fleischmann experiment is a valuable object lesson, you know, regardless of whether there is anything to the field that sort of followed their work. And to me probably the most disturbing comment that was made and left out there was the almost inference that it's O.K. to drop a data point if your name is Millikan [the physics Nobel laureate]. I don't think it's O.K. to drop a data point if your name is Millikan, Parker, Fleischmann, or Pons. That's the lesson. That is what science is about. We don't drop data points, we don't become passionate, you know, about "this has to be right, we have to make that data look this way." [***Ironically, that is precisely what the MIT PFC did with its data! —EFM***] Science is supposed to be objective, even if it sometimes goes against the grain, and that is what we try to teach here at MIT to our students. Let it come out the way it comes out and don't mount a big PR campaign, you know, and if it doesn't fit, then force the data to fit. We're trying to be dispassionate. That's what science is about and I hope that's what students will take out of this whole thing. Regardless of whether or not any of these other experiments which you can mention are right or wrong, let's look at them one at a time. Let's try to reproduce them.

We at MIT looked very carefully at Fleischmann and Pons, and this is what we came up with. [If we] think we ought to look at another set of experiments and we think we have expertise, we will. But just let it fall where it lies. We're not going to come out one way or another until we look at it.



MIT PFC Promotional Brochure

Mallove: Would you consider re-evaluating your own experiment, if I brought in experts to evaluate it? Would you consider that? Because I've asked Dr. Luckhardt for several weeks now—and I know he's not here today. He told me at one point he would provide me with the heater power curve for the light water experiment so that I could ascertain what the heck was going on in that experiment. He then finally ended up saying to me he would not give it to me—or that it would take a week to do it.

Parker: I think, Gene, that what you showed up here earlier is completely a surprise to me. [The Phase II comparison power tests of light water versus heavy water, published and unpublished versions.] We will give you every piece of data we ever took.

Parker: My personal. . .

Mallove: Fine.

Parker: I'll tell you what my opinion is of that work, because I was part of it. I don't think it's worth very much. Alright? And that's why it's just published in a tech report. I don't think it's worth very much. I think to do calorimetry is one of the hardest things I ever tried to do. I'd rather stick to plasma physics.

Mallove: But, Ron, with all due respect, I agree with you, I agree with you. [that the work was not conclusive]

Parker: When you have an open system is where you can make big errors, where you don't know the overpotential, the electrode potential, and so on. These things are unknown. I mean it's really tough **and that's why I don't put any stock at all -- you can redraw those curves anyway that you want. I don't think that data is worth anything.** Now you may be able to find something in it. I did the experiment; I don't think it's physics.

Mallove: But what I've seen, because I certainly see it from Douglas Morrison [of CERN] and I see it from people like Frank Close and others, that your prestigious laboratory with its excellent resources is being used in some respect as a standard which everyone else is supposed to adhere to. My own personal feeling is that those who have continued beyond May of 1989 to do experiments, have gotten some very significant results that this laboratory and other laboratories at MIT ought to take a look at again, and that's the only thing that will ultimately clear this up. I don't agree that passion and PR and so forth should solve this: I think experiment should, but they are not being done here.

Frank Close: Can I say something? It's one o'clock and we've got to go to a luncheon. [inaudible] I think that what Ron just said about moving data points and [inaudible]. Whether this turns out in the long run to be right or wrong is a completely separate issue as against what happened at the time. This really addresses the question of what you were saying to the students. One cannot do science and start just dropping data points because it was convenient for you, changing curves around because you wanted to prove something. If you do, and you're caught out, that's how it is and I could not rightly suppress information once it had come my way. If scientists try to hide the fact when they discover that things are being done in the name of science malevolently, then science is going to suffer for it. And if then people who come out and whistle blow get attacked for it, it's even more disturbing. We saw what happened over many years with the David Baltimore case and how long it did take for that to come out. I don't think that those sort of things will give science a very good name, if we didn't address them when they came up.

Petrasso: Thank you very much for coming today.

New Energy Times Archive

Fall 1991 (NASW's)* Newsletter ("SW")

SCIENCE WRITER QUITS MIT NEWS OFFICE, CITES COLD-FUSION DISPUTE

by Lee Edson (Reprinted with permission from NASW).
(Freelance writer living in Stamford, CT)

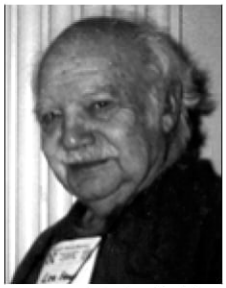


Photo by E. Mallove

"I am convinced at greater than 99% confidence level that cold fusion is real—both the nuclear emanations that have been reported and the excess enthalpy that seems to emerge from various experiments." Thus did Eugene Mallove, chief science writer of the MIT News Office, write to MIT President Charles Vest in April 1991. After detailing new, and as he put it, astounding findings in Russia and India, he decried the lack of experimental work on cold fusion at MIT "after the initial but intense period of experimental assessment in the spring of 1989."

Mallove based his expertise in large part on his research for *Fire from Ice*, an optimistic book on cold fusion published in July by John Wiley & Sons. He also holds a B.S. and M.S. in astronautical engineering from MIT, a Sc.D. in environmental health sciences from Harvard, and has authored several other scientific books.

Urging the MIT president to set up a panel to investigate the status of research on cold fusion, Mallove went on to say, "I do not feel that MIT's interests are best served any longer by unwarranted ignoring of the mounting experimental evidence for cold fusion. It seems to me essential that members of the MIT community reassess experimental findings that are coming from both foreign and domestic laboratories. To do any less would be an abdication of scientific responsibility, not to mention a longer range injury to the reputation of MIT. . ."

By June, Mallove had not heard from MIT and was convinced that the scientific community had closed its mind to cold fusion as a real phenomenon, even if not promising as a source of endless cheap energy. Frustrated and feeling uncomfortable about continuing his role as a spokesman for MIT, NASWer Mallove quit his job in the News Office, announcing his resignation at a public meeting, and submitted a 17-page "J'Accuse" letter to his alma mater. The litany of charges expanded on his earlier note to the president. He accused the university of publishing fudged experimental findings to support MIT's early condemnation of the work of Pons and Fleischmann—a condemnation he charged that helped propel the nation's negative tone toward the Utah scientists.

Mallove went on to blast MIT Professor Ronald Parker, head of the Plasma Fusion Center, for "using" him and the press office in publishing a false press release. In that release Parker had denied that he had ever called Pons and Fleischmann frauds as reported by Nick Tate in the *Boston Herald*. Tate later produced transcripts that showed Parker had indeed used the expression "fraud" on several occasions, and in a classic riposte Parker said that he didn't mean it in connection with the controversial cold fusion findings.

Mallove also charged that the university was seeking to censor his writings by killing a 9,000-word article that he had written for the MIT magazine, *Technology Review*, explaining his views on cold fusion. He claimed the article had been accepted after revision but was later turned down because of the negative comments of reviewers, especially by an MIT physicist who was violently anti-cold fusion. (In a subsequent telephone interview Jonathan Schlefer, the former managing editor of *Technology Review*, who told me he was responsible for rejecting the article, firmly denied Mallove's allegations, saying that his article was too one-sided and not up to snuff.)[Ed. Note: This reconstruction by Schlefer is utterly false—EFM]. Nevertheless, Mallove was paid the full price of $1,000 for the article.

The core of the scientific misconduct alleged by Mallove has to do with calorimetry experiments performed by Professor Parker and his group. In one calorimetry experiment a Pons-Fleischmann electrochemical cell was filled with heavy water and a control cell with ordinary water. The power curves generated were published in the *Journal of Fusion Energy* and came out looking essentially the same, apparently indicating that the heavy-water cell had not produced excess heat as might have been expected if a fusion process were going on. Mallove says that the heavy-water curve was shifted by the experimenters to make it look the same as the ordinary water curve but that actually the heavy-water cell experiment did show excess heat. Parker's explanation is that the shift was made in accordance with conventional data treatment.

All this came at time when MIT was still reeling from the Baltimore-Imanishi-Margot O'Toole furor. Perhaps that is why Mallove's resignation drew only mixed attention from the media. *The Boston Herald*, UPI, the *Chronicle of Higher Education*, and the *Christian Science Monitor* thought it newsworthy, but *The New York Times*, *Science* magazine, and *Nature*, which would normally have covered or at least noted such a dramatic form of professional self-immolation in academia, were notably silent. Interestingly the *Wall Street Journal*, which in July reported a series of new cold fusion findings (or "sightings of the dead" as the physicists regard them), failed to mention Mallove's whistle-blowing departure from MIT.

The MIT administration also did not respond, although Professor Parker said he thought the affair ridiculous and dismissed the alleged new evidence of cold fusion as no evidence at all.

Mallove then accepted an invitation from National Public Radio to air the controversy over WBUR in Boston. The broadcast of 9 August 1991 led off with: "A crisis of confidence in Boston's leading research institution . . . MIT scientists are now being charged with manipulating the media and altering data in an attempt to shoot down the work of the Utah scientists."

Then Mallove's voice cut in: "What went on behind closed doors at my alma mater is so upsetting that I will not rest until the whole matter is given thorough airing. We have a major big science program, hot fusion, which is literally trying to squash cold fusion."

In late August, Mallove pressed Parker and his group for their lab notebooks to allow for an independent check of the calorimetry work in 1989. So far, he says, only an item or two has turned up. Parker insists that it isn't worth the effort to have an assistant generate all the lab data involved. While Mallove interprets this as bad faith, the prevailing view at MIT is that rather than fraud the scientists involved may have conducted poor science that they would not like to expose.

Mallove has now escalated the furor once again by dispatching a registered letter to Mary Rowe, assistant to the president of MIT, requesting a formal inquiry into the misconduct charges. This time *Nature* did report the incident as a cold-fusion tempest at MIT. Much of the betting is that a formal inquiry is unlikely to take place, mainly because it would be hard to prove malicious intent, and even if the data had been improperly handled at a time of high tension at MIT, it would mark only a minor footnote to the now largely discredited work of Pons and Fleischmann.

Is there life beyond the MIT News Office for Mallove? Apparently yes. At 44 he intends to retain his post as a lecturer in science journalism in the humanities department at MIT and he

Edson continues on page 119

*National Association of Science Writers

New Energy Times Archive

Exhibit L
Dr. Mallove's Resignation Letter from the MIT News Office
June 7, 1991

My letter of resignation form the MIT News Office was submitted June 7, 1989, two days before my 42nd birthday. It details the constellation of concerns about unethical press manipulation and data manipulation that was the central fact of the MIT PFC's response to cold fusion.—EFM

Eugene F. Mallove, Sc.D., Engineering
Lecturer in Science Journalism, Department of Humanities
Massachusetts Institute of Technology

June 7, 1991

Kenneth Campbell, Director and
Robert DiIorio, Associate Director
MIT News Office, MIT Room 5-111

Dear Ken and Bob:

The time has come to formalize what I have been alluding to these past few weeks. Regrettably, I must tell you that I intend to leave the MIT News Office within this year as soon as I am able to obtain employment elsewhere. Circumstances surrounding the cold fusion controversy and the unfortunate way it has been dealt with at MIT leave me no choice. Furthermore, the appearance of *Fire from Ice*, has already prompted insulting attacks by those negativists—on and off campus—who think that they have a monopoly on scientific wisdom in this area.

I feel increasingly uncomfortable being the ex-officio representative of the tragic and indefensible abrogation of academic standards that has occurred at MIT in this matter. The latter characterization will prompt raised eyebrows, I'm sure, given that in the (erroneous) popular view it is cold fusion researchers who are the exclusive violators of such standards. But this amazement will merely be another manifestation of arrogance toward and misinformation about cold fusion research. Please excuse the length of this letter, which is of the nature of a report, albeit not a comprehensive one, on the treatment of cold fusion at the Institute.

This is a serious matter, not some esoteric quibbling about a peripheral exotic question. The sooner the MIT administration understands this and acts upon it, the better it will be for this cherished place of great dreams, visions, and deeds. I am proud to be an alumnus of MIT, but I am outraged, embarrassed, and amazed at what has happened here. Of course there may well be an open-minded attitude toward cold fusion among a large "silent majority" of students and faculty here. I hope that my book will be able to inform those at the Institute who still are curious about cold fusion. The most visible MIT response to cold fusion so far, however, has been an appalling arrogance and intolerance, combined with actions that have significantly hindered understanding of the phenomenon here and elsewhere. The consequences for MIT could well be devastating when the last "i" is dotted and the last "t" crossed toward proof that cold fusion phenomena exist. The shield that falsely protects the Institute now is the milieu of skepticism that surrounds cold fusion in certain prominent publications and societies, but that skepticism is doomed to collapse like a house of cards. It is only a matter of time, and it may be sooner than many believe. Ironically, this is a false shield of skepticism run amok that some researchers within MIT have labored mightily to help build.

Frankly, the direct evidence for nuclear effects in many cold fusion experiments is already overwhelming. If and when—more likely I would say, when—the measurement of real excess power production is resolved and proved to come from heretofore unknown nuclear processes, the MIT response to cold fusion will be judged most severely; and that negative assessment will be completely correct unless an immediate and dramatic change of course occurs. If cold fusion ultimately proves to be a utilitarian power source, it will be very difficult for MIT to recover its credibility.

Some of my intolerant critics will probably hasten to suggest that it is I who will suffer the consequences of a too credulous view of cold fusion. On the contrary, I will never be embarrassed by my views, first because they have been honestly reached; I started with deep skepticism, went back and forth from belief to disbelief many times, and arrived at what is to me an inescapable conclusion. Second, even were I to be proved wrong—an unlikely event—I have taken great pains to spell out precisely the required circumstances for the collapse of the multiple channels of experimental evidence that would have to occur to prove that cold fusion is an illusion. If that unbelievable circumstance should arise, so be it, but I wouldn't recommend waiting for it.

I know that there are many other dimensions of my job in the News Office that present no apparent conflict. By right, there should have been no conflict in the matter of cold fusion either—even though I have written a book on the subject that takes a contrary view to widely held skeptical opinions. After all, isn't diversity in scientific viewpoint supposed to be the driver of progress at a great research university? And I do have scientific and engineering training and experience, and am presently a Lecturer in Science Journalism in the Department of Humanities. These credentials certainly qualify me to discuss this subject as a peer of those who decry it. But cold fusion is no ordinary topic. Regrettably, it has not been possible to discuss it here as one would, for example, relativistic space travel or "child universes"—concepts that are hardly "accepted," but which apparently do not cause the visceral reaction to their mere mention that cold fusion does. As Dr. James McBreen of Brookhaven National Laboratory has said, "A lot of people undergo personality changes when discussing this topic."

Indifference, Intolerance, Ridicule, Censorship

On 12 April [1991] I wrote to President Vest about cold fusion, and sent a copy of the letter to former MIT president Gray (see attached). The letter was a summary of where I thought matters stood now in the field, including the reports of the recently announced Soviet work and the well-known Japanese involvement. I asked that Dr. Vest consider appointing a panel to assess the field in light of many new developments. I presume he has taken the matter under advisement, but I find it distressing that no hint of a response has come on this earnest appeal. I know that our chief executive is very busy, but this is an important matter. It would not surprise me at all, though, if that letter were being disparaged by high-level negativists here who are legion.

Much more disturbing is the stark reality that since the spring of 1989, no experimental work on cold fusion has occurred at MIT, an indisputable message of indifference. Thus we have the institutional response, in effect, "It's dead." One of the world's greatest scientific institutions has not actively participated in its splendid laboratories in getting to the bottom of a possible new scientific phenomenon. Incidentally, even if "cold fusion" were not to be a revolutionary nuclear process, there is broad agreement even among skeptics that some unusual thermal effects have been seen in palladium-platinum heavy water cells. So where is the scientific curiosity among our resident skeptics to put that final nail in the excess power issue by doing experiments to discover what is causing these effects—possibly interesting and useful in their own right even if not nuclear? Are our

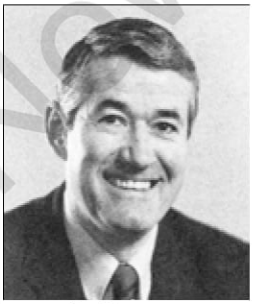
New Energy Times Archive

resident skeptics waiting for government funding? No, they don't really want to be bothered with this research. Even if they were inspired to do it, they wouldn't get the money, of course. The skeptics who influence and control DoE's purse strings have made sure of that. What follows is the untenable and unscientific position of DOE, as stated by Secretary of Energy Admiral James Watkins in a recent speech (May 6, 1991). This attitude came about not of course exclusively from, but in no small way through the efforts of influential members of the MIT community:

> "Remember cold fusion? Front page news for weeks on end. Will it work or won't it? Is it the key to our energy freedom, or a hyped-up hoax? In the end, it was neither. Just bad science. But how was the public to form an opinion when the scientific community itself and the reporters who covered the story were unable to persuasively lay out the scientific merits of the issue."
>
> "But there was damage done here too. Two members of the scientific community made everyone in white lab coats look fraudulent. Congress held hearings and railed against my agency and others for not pouring millions into cold fusion, in the process shedding no light on the real underlying issues of energy production and use. And at the same time, they cut my department's budget for real fusion energy by $50 million."

Sec. of Energy Admiral Watkins

I am aware of some relatively quiet cold fusion work done by staff members of MIT, that was conducted discretely off campus. There were also a few efforts carried out at Lincoln Laboratory. Some anomalies were seen, and interest there thankfully has not died completely. [Ed. Note: Definite excess energy later *was* observed at Lincoln Labs, but the results have been withheld from the public.—EFM] But there has been no other significant experimental work, as far as I am aware. This is disappointing, but not surprising. Professor Ronald R. Parker, one of the two professors who led the limited MIT cold fusion experimental effort from late March to late May, was quoted in his letter to author Robin Herman (*Fusion: The Search for Endless Energy*, 1990). Her book has a concluding chapter mocking cold fusion in which Professor Parker is quoted. "Unfortunately, a lot of time and effort has been wasted due to this blunder." I was amazed to discover that when this statement was made (May 11, 1989), the experiments to explore cold fusion at MIT had not even been completed.



Robert Birgeneau

Though there has been no laboratory work since the spring of 1989, cold fusion has been the butt of jokes and the focus of merriment at MIT. At the Plasma Fusion Center in the summer of 1989, a "Wake for Cold Fusion" party was held. One of the MIT reviewers for a major publication [*Nature*] that has blocked numerous attempts by researchers with positive cold fusion results to publish them, once was known in this area by an editor of that publication as, "Rambo." [This was Dr. Richard Petrasso of the MIT PFC] The head of the Physics Department [Prof. Robert Birgeneau] remarked with humor and pride in the summer of 1989 department newsletter, "I should like to note, however, that none of our faculty contributed to the confusion surrounding 'cold fusion.'"

There have been other remarkable comments. "Garbage" was how one MIT physics professor [Prof. Martin Deutsch] bluntly characterized cold fusion work to a prominent science magazine [*Science News*] in 1989. One of the researchers who was on the Plasma Fusion Center/Chemistry Department team evaluating cold fusion [Prof. Ronald Ballinger] told me five months ago that he thought Pons and Fleischmann were "crooks who should be put in jail." Another team member, Dr. Richard Petrasso, was quoted recently on the front page of the *New York Times* (March 17): "I was convinced for a while it was absolute fraud. Now I've softened. They [Pons and Fleischmann] probably believed in what they were doing. But how they represented it was a clear violation of how science should be done." Apparently another skeptical physicist could not sanction that severe charge. In a letter to the *New York Times* printed April 9, 1991, Yale physicist Robert Kemp Adair wrote: "Last November, I served on a a committee that met with Dr. Pons in a review of the National Cold Fusion Institute at the University of Utah. Though I concluded that he and Dr. Fleischmann had seen no cold fusion, I am confident they reported no invented data and committed no egregious breach of scientific ethics." Unfortunately, the insinuation of fraud applied to these researchers was given early impetus here.

Recently, the attacks took an uglier turn both from within and from outside MIT. Physicists Dr. Frederick Mayer and Dr. John Reitz of Ann Arbor, Michigan—both with distinguished scientific careers—were invited to MIT by Professors Peter Hagelstein and Lawrence Lidsky—to conduct a scientific seminar about their theory of "cold fusion" that appeared in their "Nuclear Energy Release in Metals" paper, which has been published in *Fusion Technology*. The seminar was advertised in the usual channels on campus. To my knowledge, since the affair began it was the first technical seminar on cold fusion open to the general public at MIT that actually cast the phenomenon in a positive light. The presentation was informative and was conducted with dignity. I was proud to have helped facilitate this meeting—a worthy effort, I thought, to clear the air on the topic. In advance, Dr. Mayer had expressed a fear that he would be scurrilously attacked, as opposed to being challenged with reasoned arguments—as he hoped he would be. Since Dr. Mayer is an acquaintance of President Vest (their sons are friends too, and Dr. Mayer was at one time a soccer coach of President Vest's son's team), that would have been especially offensive.

Fortunately, Dr. Mayer was not attacked at the seminar because those most likely to offend didn't show up; the critics held their fire until afterwards. Apparently Dr. Robert L. Park of the American Physical Society's Washington office took offense not only at the theory presented by these scientists, but at their press conference in the Boston Sheraton Hotel the day after the seminar at MIT. (Only three members of the media attended.) Park, who has mocked cold fusion from the beginning, much as his weekly electronic mail column, "What's New," ridicules those who study the possible effects of low frequency electromagnetic fields on biological systems, was also upset that I had provided nominal assistance (on my own time) for the Reitz-Mayer press conference.

New Energy Times Archive

What was the nature of this aid? It was merely to fax a news release (which they had prepared) and their technical paper to a handful of media outlets; I also made a few phone calls, and was contacted by people who had heard about the press conference, including Park's secretary. That's how he was informed to write his diatribes. My news judgement then and now was that the seminar at MIT reflected well on the Institute; it showed that we are at least nominally in the business of openly discussing even controversial scientific matters. Also, by facilitating news stories that reporters apparently found interesting (such as William Broad of the *New York Times*), the News Office maintained its deserved reputation as a useful information outlet. I would make that same judgement again about any other topic. Occasionally some interesting figure from outside MIT arrives to make a controversial statement, *e.g.* scientist Dr. James Lovelock (the Gaia hypothesis), and our office doesn't hesitate to "promote" them to the press.

As a result of these highly appropriate scientific events, these are the "gifts" we received from Park. They came in two successive weeks of his widely circulated column, which is signed "Robert L. Park, The American Physical Society," giving the impression that his is an official Society view, even though it is not. Not once did Park mention the scientific seminar at MIT. He preferred what he evidently considered to be the pejorative "press conference." Here is the 1st message:

Dr. Robert L. Park

"INCREDIBLE COINCIDENCE: SIMULTANEOUS BOSTON PRESS CONFERENCE! At the very instant that Mills was revealing his startling new findings in Lancaster, two-well known physicists, Fred Mayer and John Reitz, were in Boston announcing their new cold fusion theory, with the help of the MIT press office. Their paper, which will also be published by *Fusion Technology*, involves—are you ready?—tiny hydrogen atoms! Except they call them 'hydrons' and attribute them to 'continuum bound state resonances.' Mayer expects prototype power generating systems in about five years. Neither Mayer or Reitz is associated in any way with MIT. How then did the MIT press office get involved? Very good question?" (April 26, 1991)

Dr. Fred Mayer

Professor Ronald R. Parker of the PFC chose not to bring this piece of slander and omission directly to my attention. Evidently he agreed with its tenor and was stirred up about the Mayer-Reitz press conference. Instead, he faxed a copy of it to someone in the MIT News Office, who has little familiarity with the scientific issues of cold fusion and with whom you know I have had clashes.

In the *Washington Post* on Friday 26 April, Park made this statement about Mayer after describing his theory as "wacky": "There is no reason to doubt the sincerity of the two scientists involved, who are respected and well known as science managers [note the put-down "managers"—the two are practicing physicists!] But there are also sincere scientists who believe in psychokinesis, flying saucers, creationism, and the Chicago Cubs."

Continuing his coordinated attack, Park made this insulting statement to the Chronicle of Higher Education--his assessment of the Mayer-Reitz theory: "It is proof again that a degree in science is not an inoculation against foolishness and mendacity. It's just got to be wrong."

The following week, Park attacked me in his column again, this time directly:

> "MIT FUSION FLAKE FLACKS NEW BOOK! TINY LITTLE HYDROGEN ATOMS called 'hydrons,' explain cold fusion, according to two Ann Arbor physicists who held a press conference in Boston last week. Why was the press conference in Boston—and why was the MIT press office helping? The answer seems to be that an MIT science writer is promoting his new book, which contends that the evidence for cold fusion is persuasive. He predicts that in the history of science Pons and Fleischmann will be viewed as heroes."

The major falsehood of this nearly libelous statement: at no time during the Mayer-Reitz visit to Cambridge-Boston was *Fire from Ice* mentioned publicly, in any context. What a stupid way for me to "flack" a book! Park comes closer than anyone I know to wearing well the term: "scientific bigot." It was he who in March 1990 described the First Annual Conference on Cold Fusion in Salt Lake City as a "seance of true believers"—without having attended the meeting or learned what went on.

Now, despite their outrageous intent to defame and ridicule, one could dismiss the mouthings and electronic missiles of Robert Park as the pathetic prejudice of an aging physicist, who may fear that his world view is crumbling—as the history of science has shown happens time and again. Perhaps all wisdom does not reside in the APS, Park may be thinking. However, it is unfortunately not so easy to dismiss the outright censorship of one's writings, particularly when that censorship is influenced by another physicist, this one at MIT.

Over many months I had prepared a lengthy (9,000 word) feature article on cold fusion that was to appear in *Technology Review* at the end of this summer. The piece recounted the essentials of the cold fusion story (see attached draft), by presenting the arguments on both sides, though it did come to the general conclusion that cold fusion might well be real, given the accumulating evidence. My article had passed through a major revision cycle, in which I had carefully adhered to the wishes of editor Jonathan Schlefer. In mid to late April the word came back from Jonathan that the revision seemed to be fine—not to worry. Someone else within *Technology Review* even told me it was being considered as a cover story. It was considered that good.

Imagine my shock on May 9, when I received a call from Schlefer telling me that the article was not going to be published. He offered no suggestion of any changes that could make it acceptable— the usual option when an editor has some new problem with a piece, particularly after he has professed to find it basically satisfactory. Schlefer told me that it had been sent out for review to three technical people, each of whom allegedly had some problems with it, though these problems were not clearly indicated nor were they discussed. There was a blanket statement that each reviewer had found the piece too positive. Further investigation on my part determined that, except in one case, this was far from true.

A senior and respected MIT physicist seems to have been mainly responsible for scuttling the article [Prof. Herman Feshbach]. I called to ask him what he had found objectionable. Despite my distress that the article would not appear in *Technology Review*, throughout the telephone conversation I was calm and polite. His evident anger increased through the call. He began by saying that fundamentally my article was "not a piece

New Energy Times Archive

Prof. Herman Feshbach
MIT News Office

of journalism." He described it as more of an "advocacy piece." This assessment struck me as very peculiar for three reasons: 1.) I had deliberately balanced both sides of the controversy as closely as possible, even as I interjected my own view by suggesting that it was difficult to explain away all the phenomena—that some unexplained cold fusion phenomena seemed truly spectacular; 2.) *Technology Review* is a forum for advocacy—sometimes not even advocacy with which many readers will identify. Many if not most pieces in *Technology Review* are advocacy pieces, some quite markedly so—like the cover story that suggested that the U.S. used atomic bombs against Japan to demonstrate a threat against the Soviet Union; like the recent cover that all but ratified the threat of global warming (even though the question is still being debated); or like the most recent cover story that suggests that the motivating force for the war in the Gulf was the Pentagon's need to check out a new military paradigm; 3.) Finally, it became completely clear in the remaining conversation that an advocacy piece against cold fusion would very much have pleased this professor.

I was astounded that this professor had not learned that arguments "from authority" don't hold water. He ridiculed me citing "that Bulgarian chemist." He did not use the chemist's name; it happens to be Dr. Vesco Noninski, a multi-talented electrochemist from Bulgaria who is fluent in many languages and who is visiting the United States. Noninski, apart from his having carried out in Bulgaria very novel heat measuring experiments on cold fusion—ones that demonstrated excess power—has published and prepared very interesting analyses of the MIT and Caltech cold fusion calorimetry experiments. These indicate the possibility that these teams may have measured excess power, but didn't realize it because of improper analysis of their own data. These analyses are very convincing. They don't prove, of course, that cold fusion is real, but they do indicate that not all negative result experiments may be truly null. Furthermore, Noninski is far from an "advocate" of cold fusion. He maintains that he does not know what the phenomenon is, but insists that careful calorimetric (heat measuring) analyses must be carried out even to begin to discuss "cold fusion." He has considered the error source aspects of cold fusion calorimetry more than anyone I have encountered in the field.

So what did our physicist non-calorimetry expert say about Noninski? He challenged me angrily, "You would trust this Bulgarian chemist over what Mark Wrighton said [about cold fusion]?" I didn't answer him on that point, but yes, I would, because Noninski has put far more time into the problem (see account below). The physicist was not saying "Dr. Noninski"; I kept hearing "this Bulgarian." And MIT is supposed to stand for cultural diversity? This is incredible! I could hardly believe what I was hearing from this man whom I had admired and respected. McBreen of Brookhaven was right!

The physicist [Feshbach] told me that he had "50 years of experience in nuclear physics and I know what's possible and what's not." I brought up several other names of physicists who had initially been skeptical of cold fusion, but who had done experiments of their own that had convinced them something was going on. One was at Los Alamos National Laboratory, Dr. Howard Menlove. Almost before I could get a researcher's name out of my mouth, I was cut off by an angry objection from the physicist, generally of the character, "I don't know who he is!" Finally, I tried to suggest that it might be a good idea if my critic examined a recent technical review of the entire cold fusion field that has been prepared by physicist Dr. M. Srinivasan of the Bhabha Atomic Research Center, one of India's premier nuclear facilities. Srinivasan is the head of BARC's Neutron Physics Division, but the physicist would hear no more about him. He was getting too angry to stay on the line much longer. He blurted out, "I don't want to see any more evidence! I think it's a bunch of junk and I don't want to have anything further to do with it." With that, the conversation ended.

This arrogance is so offensive that it can hardly be suffered, and it need not be. That is one of the reasons I'm leaving the News Office. I'm profoundly embarrassed that we have such close-mindedness here on scientific issues. I cannot represent such attitudes even in an ex-officio capacity. Furthermore, I intend to attack them, not only after I leave this office, but in my remaining time here.

An Unfortunate News Release

MIT always speaks the truth in press releases emanating from the News Office, right? Wrong—not always, sorry to say. On May 1, 1989, the News Office issued a press release, a copy of which is attached; it was prepared by me in telephone consultation with Professor Ronald Parker of the PFC at his request. The press release was crafted to deny statements that *Boston Herald* reporter Nick Tate had attributed to Professor Parker, which attacked Pons and Fleischmann in the manner denied in the press release. When I prepared that release, and for more than a year afterward, I believed that the statement that our office had issued was valid, though I had some doubts. I simply trusted what was being asked of me in preparing the release. I trusted too much—another reason for my leaving the News Office. I do not wish to be put in that compromised position again. My own integrity is too important.

After having listened to the actual tape of the interview that Nick Tate at long last provided me (in July 1990), I believe that his story's characterization of what Professor Parker and Professor Ballinger had said in their conversation with Tate the week before, is substantially correct. It was a well-orchestrated attempt to condemn the work of Pons and Fleischmann, not merely to criticize it technically. In my view, virtually any other competent reporter would have written essentially the same story that Tate wrote. In fact, the partial transcript published by the *Herald* on May 2, 1989, should have been enough to convince me of the soundness of Tate's story, had I not been told that these remarks were "out of context." Parker and Ballinger may deny it, but I know what I heard on that tape, which I was deeply disturbed to listen to. I have been wrestling with the knowledge of what it held, and this has been cutting me up inside ever since first hearing it.

I have a doctorate in engineering, and I came to my career in science writing at no small sacrifice in compensation, simply because I enjoy writing more and I believed that it would give me greater reach. It definitely has. I also have been proud to represent my alma mater and its fine research, including the many outstanding accomplishments in magnetic confinement fusion at the Plasma Fusion Center. Everyone, including Professor Ballinger and Professor Parker, knows that. I had, and at some level still do have affection for the people at the Plasma Fusion Center. They are personable and they have a good cause, which I am solidly behind—as I spell out clearly in *Fire from Ice*, even as I strongly believe in the prospect of cold fusion. Hot fusion research has many merits that go far beyond the distant and admirable goal of commercial power reactors, and these may

New Energy Times Archive

admirable goal of commercial power reactors, and these may still be necessary; we don't know enough to say no yet. But I am absolutely outraged to have played a role in this public deception that involved such objectionable language and accusations. I hope that an instance like this never occurs again for any member of the News Office. I officially withdraw my approval of the MIT news release in response to the *Herald* story.

More important than any feelings I have about being deceived was the effect that the intended disparagement of Pons and Fleischmann's work had on the future course of cold fusion research. Yes, others would have jumped in anyway to assail Pons and Fleischmann; the chorus might have been equally loud. But sending out the kind of calculated, negative message against Pons and Fleischmann was wrong. To add insult to injury, members of the PFC staff act as though Pons and Fleischmann were the only ones capable of what they claim to be irregularities in research. Unfortunately, their own house is not in such fine order (see below).

Further actions were taken by various members of the PFC to assist in discrediting Pons and Fleischmann. In particular, there was clearly significant cooperation with physicist Frank Close, who has written in my view a highly negative and extremely imbalanced account of the cold fusion story—his book, *Too Hot to Handle*. It focuses on numerous alleged departures in scientific ethics on the part of Fleischmann and Pons, while saying virtually nothing about or belittling all subsequent experimental work that provides supporting evidence for cold fusion, *e.g.* physicist Howard Menlove's neutron burst detection work at Los Alamos National Laboratory is not discussed. The dedication to Close's book reads precisely: "To the xxxxx from MIT and the friends and colleagues who shared the spring of 1989." [The "xxxxx" was Richard Petrasso] Incidentally, Close is uniformly disparaging of the role of the media in the cold fusion saga, paralleling the attitude of some on the PFC staff (and Park of the APS) who continue to get upset whenever cold fusion is given some credence in the media. When cold fusion is disparaged, it's fine with them. Close shamelessly brings up several times the now thoroughly discredited accusation by another journalist that fraudulent adulteration of experimental cells with radioactive tritium occurred at Texas A&M University. An interesting aside: Close is so reckless in his treatment that he was able to confuse the *Boston Globe* with the *Boston Herald*, which confusion is sure to please neither paper.

Questions About the MIT Cold Fusion Calorimetry Experiments

The MIT Plasma Fusion Center/Chemistry Department experimental contribution to cold fusion research was conducted from late March 1989 through late May 1989. The search for evidence of cold fusion in heavy water cells and comparisons with light water cells included attempts to find various nuclear products, as well as indications of excess power. The final report of the sixteen-member team appeared as Plasma Fusion Center Report, PFC/JA-89-34, dated July, 1989: "Measurement and Analysis of Neutron and Gamma Ray Emission Rates, Other Fusion Products, and Power in Electrochemical Cells Having Pd Cathodes." The authors are listed as: D. Albagli,[1] R. Ballinger,[2,3] V. Cammarata,[1] X. Chen, R. Crooks,[1] C. Fiore, M. Gaudreau, I. Hwang,[2,3] C.K. Li, P. Linsay, S. Luckhardt, R.R. Parker, R. Petrasso, M. Schloh,[1] K. Wenzel, and M. Wrighton[1] [1 = Dept. of Chemistry, 2 = Department of Nuclear Engineering, and 3 =Department of Materials Science and Engineering]. The abstract to the report concludes, "Within estimated levels of accuracy, no excess power output or any other evidence of fusion products was detected." Later this report was reprinted in essentially the same form in the *Journal of Fusion Energy*, June 1990, Vol.9, No.2, pp.133-148.

Here I wish to comment not on the nuclear product measurements discussed in this report and subsequent paper, but on the power measurements. In fact, one experiment in the series of experimental cells reported in this work is of particular interest, because it is the only case in which graphs of the raw data that form the power measurements are shown. This so-called "Phase-II" calorimeter experiment compared the power production of a light water control cell and a heavy water cell. The record of the controversy clearly shows that at that time skeptical scientists were placing great emphasis on the need to find differences in the power production between light water cells and heavy water cells. The presumption by many at the time and subsequently was that if a heavy water cell produced excess power and a light water cell of identical form did not, then there was more reason to investigate further the possibility that unknown nuclear reactions might be occurring.

A Possibly Incorrect Power Analysis

Regardless of how the data from this experiment are interpreted, I am firmly of the opinion, as are many others, that it is not possible to use the latter MIT experiment, or even the more crude series discussed earlier in the PFC paper, to conclusively prove anything one way or another about the reality of cold fusion. It is simply far too limited a check. Some other laboratories that have obtained sporadic positive results for excess power have generally spent much more time in their trials and tried a greater series of electrodes, a possible requirement sometimes to pick up the anomalous thermal effects.

However, there has been a technical analysis of this MIT experiment, which leads me to believe that at least in the heavy water case in the Phase-II experiment, there is evidence of excess power production. The evidence of excess power production based on this analysis is to appear in a forthcoming issue of *Fusion Technology*. In that paper, the power density produced in the palladium electrode rises from zero at 20 hours into the test to close to 2 watts per cubic centimeter at 100 hours.

The analysis on which this conclusion is based differs from the MIT analysis because that analysis has introduced an adjustment to the raw data whose validity remains uncertain. (This adjustment is a subtraction from the raw data of a linear fit to the noisy and declining heater power, with no quantitative assessment as to why this should be done.) In fact, if this adjustment is performed, for the reasons suggested in the MIT paper, it is possible to get a null result for the excess power measurement in every case. It is surprising that this was done, because in the methodology of the MIT calorimeter, declining heater power should have suggested the presence of an unknown heat source. The explanation in the paper does not seem satisfactory.

Without question, more work needs to be done to decide what analysis of the Phase-II experiment is appropriate. Other than that, the result cannot be used to draw any firm conclusions about excess power.

An Unwarranted Curve Shift

There is another aspect of the Phase-II excess power experiment that is troubling. Even if the MIT thermal analysis (which sanctions the forementioned subtracting out of the heater power) is presumed to be correct, data provided to me in the summer of 1989 show that there was likely to have been a difference between the heavy water cell and the light water cell; the heavy water water cell seems to be evolving excess power, while the light water cell does not—exactly what many wanted to see at the time as an indication of anomalous nuclear effects.

New Energy Times Archive

Attached are four figures. The first two are from the published MIT paper and show the excess power produced by the Phase-II heavy water and light water cells. The raw signal was noisy and was averaged in hour-long intervals to produce the data (black dots) seen in these figures. The results rise and fall above the zero excess power line, and there is nothing that leaps out from this data comparison to suggest that excess power is being produced in the heavy water cell and not in the light water cell. It looks as though both excess power plots are about equally noisy. This data that form these curves was prepared at least as early as July 13, 1989, because I was given a draft article by the PFC that bears that date. [Ed. Note: See page 74.]

On the other hand, I also was given the processed but unaveraged, and hence more noisy, data that went into forming these published curves. These data appear in the two other attached figures. The figures are copies of graphs that appear in another PFC draft report to me on calorimetry, dated July 10, 1989, three days before the draft with the averaged data. The light water graph oscillates above and below the zero excess power line (which I have introduced as a dotted line to make the comparison more clear), with no obvious bias above or below the zero line. There appear to be cyclic (24 hour?) variations in the presented excess power, but it is not clear what these are from. The heavy water curve by contrast is dominantly above the zero line, indicating the strong possibility of a residual excess power (even though the magnitude of the variation may be below the stated sensitivity, 40 milliwatts). The two curves are simply quite different. There could be something like a few tens of milliwatts excess power here, on average, as one PFC researcher agreed there could be. For this 0.1 centimeter diameter electrode, 9 cm long, 20 milliwatts would translate to excess power of 0.28 watts per cubic centimeter.

So why do we see no evidence of this possible excess power in the graphs that are in the final report and the published paper? The inescapable answer seems to be that the averaged data for the heavy water was moved down an arbitrary amount so that it now has more the appearance of the null result in the case of the light water averaged data. Interestingly, the light water averaged data seem to be consistent in level with the corresponding curve of raw processed data, that is, it has not been moved down.

I am planning to prepare an article for *Fusion Technology* that will address some of these data analysis issues.

Lessons

The recent turn of events in the David Baltimore-Imanishi Kari-Margot O'Toole affair offers some lessons for MIT on the matter of cold fusion. This is brought home most effectively by Dr. Baltimore's recent apology to Dr. O'Toole. As quoted in *The Tech* (May 5, 1991), Baltimore said, "I recognize that I may well have been blinded to the full implications of the mounting evidence by an excess of trust, and I have learned from this experience that one must temper trust with a healthy dose of skepticism. This entire episode has reminded me of the importance of humility in the face of scientific data." Clearly many MIT scientists who have recklessly attacked honest efforts to come to grips with a possible new phenomenon have lacked "humility in the face of scientific data."

Another pertinent comment was recently made by Professor William F. Schreiber of Electrical Engineering and Computer Science. Writing on the Baltimore affair in the MIT *Faculty Newsletter* (April 1991), he said: "A name on a paper implies responsibility for its contents. We certainly ought not to appear as authors of work we have not watched carefully enough to know whether or not it is correct."

Employment Prospects

[*Editor's Note: non-relevant sections of this part of resignation letter omitted for brevity.*]

. . .Working at the MIT News Office as Chief Science Writer has been a great privilege and an enlarging experience for me. The vistas that have opened up are immense and the talented people and friends I have come to know are many. I wish that the MIT community had been able to react with less acrimony and divisiveness in the matter of cold fusion. I will not reconsider my decision to leave the News Office unless that situation changes radically, something I do not foresee happening soon. But I am deeply grateful to both of you for having selected me in the summer of 1987 to fill the important role of science writer, and I appreciate that you have always respected my abilities and sought my perspectives. But circumstances dictate a moving on. Whatever may transpire, I hope to stay in touch with you and perhaps even work with you in some new capacity in the future.

Science Reporting Suggestions

[*Editor's Note: non-relevant sections of this part of resignation letter omitted for brevity.*]

. . .Difficult as some of these matters are to hear about, I hope this airing of views has been helpful to you and will lead to beneficial changes within MIT. You have been great people to work with. (Even though this letter is being given to you today, it was written in nearly its present form on May 24, 1991).

Sincerely, Eugene F. Mallove

Exhibit M

Eugene Mallove's Formal Request for MIT PFC Raw Data

June 14, 1991

Following my resignation from the New Office, I attempted, in vain, to get the data that Prof. Parker had promised me at the public forum on June:

To: Professor Ronald Parker, Director
Plasma Fusion Center, MIT

From: Dr. Eugene F. Mallove, Bow, New Hampshire
Date: June 14, 1991
Re: Data Required for Further Evaluation of the MIT Cold Fusion Calorimetry Experiment

In response to your offer to provide data that I might request concerning the cold fusion calorimetry experiments carried out in the spring of 1989 at the PFC, I would appreciate receiving the following items:

(1) The unpublished Phase-II experiment heater power curve for the H_2O case, corresponding to the D_2O heater power curve that was published.

(2) Copies of all laboratory notebook pages relating to the PFC calorimetry experiments on cold fusion, both Phase-I and Phase-II.

(3) An explanation of why the hour-interval-average excess power curve for the case of the D_2O Phase-II experiment is centered around the zero excess power level, when the processed data (before time- averaging) on Dr. Luckhardt's memo of July 10, 1989 are almost entirely above the zero excess power line. A memo dated July 13, 1989 is where this apparent change occurs, and that is the graph that was published.

(4) An exact data-processing and mathematical description of how the excess heater power curves were arrived at from raw experimental measurements.

New Energy Times Archive

(5) Calculations, if any, that provide a thermal analysis for heat flow out of the top of the cell, including the glass tubes, etc., which touch the cell solution.

My sincere thanks in advance for any information that you might provide on your experiments. Some of my colleagues may be interested in the nuclear products data, but I am concerned only with the heat measurements.

Sincerely, Eugene F. Mallove

Exhibit N
Eugene Mallove's Letter to Professor Parker
July 30, 1991

The data and notebooks promised and requested were still not forthcoming from Prof. Parker as of July 30, so I sent him a reminder—not really expecting to get satisfaction from this stonewaller.

Professor Ronald Parker, Director
MIT Plasma Fusion Center, Room NW16-288

Dear Professor Parker:

It is surprising and distressing that six weeks after my written request to you and your colleagues for data and information that you publicly promised to provide about the PFC cold fusion experiments, not a single requested item has been sent to me. I hope that you will remedy this situation very soon or at least explain the reasons for the delay. I have attached a copy of the fax to you of 14 June 1991, which lists the needed information.

Sincerely, Eugene F. Mallove

cc: Kenneth Campbell, MIT News Office

Exhibit O
Prof. Ronald Parker's Letter to Eugene Mallove
August 8, 1991

By a "miraculous coincidence" a letter from Parker arrived by fax at my home on August 8, 1991, the very day before the Broadcast of the August 9, 1991 WBUR radio program by David Baron concerning the cold fusion furor at MIT and my resignation (see Exhibit P, p. 101). Of course, since Parker had been interviewed for this program, he knew it would be broadcast, though he could not know how revealing it would be of his various perfidies against cold fusion. However, no data—on request since June 14, 1991—accompanied Parker's fax. This letter is an insult to the intelligence of any scientist who thinks about it. Here we have Parker post-experiment reconstructing the objective of the PFC Phase-II calorimetry so that it coincides with his hoped-for outcome, namely a null result!

PLASMA FUSION CENTER
MASSACHUSETTS INSTITUTE OF TECHNOLOGY
Cambridge, Massachusetts 02139
Ronald R. Parker, Director

Dr. Eugene F. Mallove, Bow, NH

Dear Gene:

Regarding the specific information that you requested in your June 14, 1991 fax, I will attempt to respond to your point (3), the same point which you raised after the seminar by Frank Close, and, as I understand it, the same point you raised with Stan Luckhardt who gave you the same answer as I will give you here.

As the calorimetry experiments progressed, electrolyte evaporated causing a decrease in thermal conductivity of the system to the external world and a concomitant decrease in heater power required to sustain constant temperature. The difference in the two curves corresponds to two different ways of accounting for this systematic baseline drift. In one, the drift was fitted with a somewhat arbitrary linear function, and the data in Figs. 4b and 5b of the 7/10/89 draft was produced after subtraction. In the other, the drift was fitted with a different linear function, this time a least-squares fit, and the data appearing in the final version of the paper were produced. The difference in the two results is an indication of the error intrinsic in the measurement. The implicit assumption was that we were looking for a fast turn-on of the anomalous heat production and so it was legitimate to subtract out a slow baseline drift caused by depletion of the electrolyte. Whether this is a correct assumption is arguable, but in any event the main conclusions stand: We detected no significant difference between H_2O and D_2O, and in both cases any excess power would have been less than 79 milliwatts, the level claimed for a similar experiment by the Utah group. Our paper estimates the uncertainty of calorimetry measurement as 40 mW, and so you are free to posit an excess heat less than this level it you wish.

As for the other points raised in your fax, I believe that Stan has been extremely forthcoming in discussing them with you, and would be willing to assist you further. However, he has major responsibility for several PFC projects and I cannot allow him to take time away from, them for this purpose. I suggest that you negotiate directly with him to see what arrangements could be made.

Sincerely, Ronald R. Parker

cc: Ken Campbell, MIT News Office

MIT Aero/Astro student Ray Conley, works on his cold fusion project in the Dept. of Aeronautics and Astronautics. His patent application was attacked by the USPTO, in part by the citation of the "negative" MIT PFC cold fusion experiment of 1989 (See *Infinite Energy*, No.11).

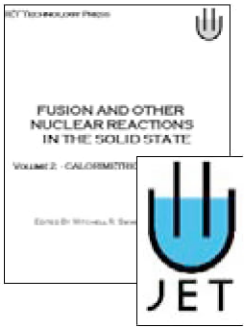
Photo E. Mallove

FUSION and OTHER NUCLEAR REACTIONS IN THE SOLID STATE

Volume 2 - Calorimetric Complications [Includes: "LESSONS FROM OPTICAL EXAMINATION OF the PFC Phase-II CALORIMETRIC CURVES"]
Edited by Dr. Mitchell Swartz; JET Technology Press (Wellesley, MA)
ISBN 1-890550-02-7, March 1999
(60 pages, 8 color figures) $24.95
The book is part of a series.

BUT for subscribers of *Infinite Energy*
$19.95 (available~April 99)
(shipping/handling $2.00 U.S., $4.00 outside US)

Order from: JET Technology
P.O. Box 81135
Wellesley Hills, MA 02481



New Energy Times Archive

Exhibit P Transcript of WBUR (90.9 FM)
Radio Broadcast,
Friday, August 9, 1991, 5:50 a.m. and 7:50 a.m., Boston, MA.
[WBUR is a National Public Radio affiliate station.]

In disgust with all that had happened against cold fusion, I resigned from MIT on June 7, 1991. Later that summer, August 9, after I battled with the Plasma Fusion Center to get data that Parker had promised me on June 7 (see Exhibits K, M, and N), science journalist David Baron broadcast over Boston's National Public Radio affiliate, WBUR, a program that encapsulated the controversy at MIT up until then.—EFM

A crisis of confidence in Boston's leading research institution, details just ahead here on 90.9 WBUR Boston at Boston University. I'm Jordan Weinstein. . . [weather report and lead-in music]

Announcer: One of the Massachusetts Institute of Technology's top spokesmen on scientific matters has resigned, charging researchers at the school with misconduct. The dispute centers around events of two years ago during the flurry of activity stemming from claims of cold fusion by researchers at the University of Utah. MIT scientists are now being charged with manipulating the media and altering data in an attempt to shoot down the work of the Utah scientists. In our weekly segment on science and health, WBUR's David Baron reports:

David Baron
WBUR Science Broadcaster
Boston University Photo Services

Baron: For almost four years, Gene Mallove represented MIT to the world. As chief science writer for the Institute, Mallove produced press releases and encouraged reporters to write stories about the school. With a bachelors and masters degree from MIT and several books to his credit, Mallove was considered well-qualified for the job. Two months ago, he quit.

Mallove: What went on behind closed doors at my alma mater is so upsetting that I will not rest until the whole matter is given thorough airing.

Baron: In a seventeen-page resignation letter, Mallove alleged misconduct by scientists at MIT's Plasma Fusion Center, misconduct he says he unearthed while researching a book on cold fusion. MIT's Plasma Fusion Center receives tens of millions of dollars in federal funds each year to develop an energy source based on hot fusion, the same process that powers the sun. Mallove says the scientists had a vested interest in seeing cold fusion die as quickly as possible.

Mallove: We have a major Big Science program, hot fusion, which is literally trying to squash cold fusion.

Baron: For most people, the cold fusion story began on March 23, 1989. On that day, two chemists at the University of Utah announced that they had accomplished in a simple table-top experiment what other scientists had failed to do after decades of work. Stanley Pons and Martin Fleischmann said they'd produced sustained power using nuclear fusion—potentially providing a cheap, safe, and nearly inexhaustible source of energy. Scientists worldwide raced to replicate the Utah work. Some laboratories initially reported success, but the excitement soon turned to skepticism when many of those early claims proved incorrect. Then on May 1, 1989, the *Boston Herald* ran a story on its front page with the banner headline: "MIT Bombshell Knocks Fusion Breakthrough Cold." The article said that MIT scientists had discovered serious flaws in Pons and Fleischmann's work. Ron Parker, head of MIT's Plasma Fusion Center, reportedly accused the Utah scientists of misrepresentation and maybe fraud and called the research "scientific schlock."The story immediately spread across the country. MIT issued a press release, denying that Parker had made the published comments. Parker spoke at a news conference that day:

Tape of Parker's remark at 1989 press conference: Let me just say quite clearly for everybody, that I am not, have not, and, uh, really seriously doubt whether I ever will accuse Professors Pons and Fleischmann of fraud.

Baron: Parker says he was misquoted. The *Herald* reporter who wrote the story, Nick Tate, says when he heard that, he was flabbergasted.

Tate: Not only were the concerns about possible misinterpretation and fraud and scientific schlock repeated to me more than one time during the course of our interview, but I also had them on tape.

Baron: At the time, Tate decided not to release his recording of the interview, but a year later he gave a copy to Gene Mallove at MIT. And this week he gave a copy to WBUR. On five separate occasions during the taped interview, Parker uses the word "fraud." At one point he says, quote, "Now it transcends the question of whether they misinterpreted to the question of whether there was deliberate fraud."

Tape of Parker talking in 1989 interview: . . .you know now it transcends I think the question of whether they misinterpreted, to the question of whether there was deliberate fraud.

Baron: At another point, Parker says to reporter, Tate, quote, "So what are you going to do with this, Nick? What you're hearing is we think it's a scam, right?"

Tape of Parker talking in 1989 interview: What you're hearing is we think it's a scam, right?

Baron: Elsewhere in the interview Parker indicates he purposely did not contact the *Boston Globe* with his story, because that newspaper was, he said, "leading the cheers for cold fusion." The way Gene Mallove sees it, Parker and colleague Ron Ballinger, who also took part in the *Herald* interview, planted the critical story in the *Herald* to generate widespread negative publicity regarding cold fusion. MIT's Ron Parker says he stands by his claim that he was misquoted. He points out that the *Herald* said he accused Pons and Fleischmann of misrepresentation. The word he really used was misinterpretation.

Parker: I have said that they have published data which was incorrectly interpreted and not real data. Uh, I, have, uh, no way of knowing if they knew at the time or even since that time whether the data was incorrect.

Baron: Parker says his comment about a scam was meant to refer not to Pons and Fleischmann's research, but to their request for $25 million from Congress. And Parker denies he was trying to manipulate news coverage. He says he was merely trying to communicate important findings about the Utah work.

Parker: When we had found out that they had made fundamental mistakes in interpreting their data, we felt certainly an obligation to bring this to the attention of the media at that time.

New Energy Times Archive

Baron: But when Gene Mallove resigned from his post at MIT, he said Ron Parker's unethical behavior went beyond the episode with the *Boston Herald*. In July of 1989, Parker and his colleagues reported the results of their attempt to replicate Pons and Fleischmann's work. They claimed they found no evidence of fusion, either in the form of radiation or energy produced. But when Mallove recently found a draft of that final report, he discovered what he considers a significant discrepancy. One of the graphs in the draft report suggests some low level of power was produced, but in the final version, the points on the graph have been moved.

Mallove: The data has been shifted down to make it look like there was no net power. Now in my estimation, there was absolutely no justification for this and I do not know why this was moved, nor have I had an answer from them as to why it has been moved.

Baron: Mallove suspects the data was moved because the scientists didn't want to provide any positive evidence for cold fusion supporters. But Ron Parker says it was unclear exactly where to put the data points on the graph and in the final report he used a more sophisticated method for determining their placement. Besides, he adds, the level of power hinted at in the earlier graph falls within the experiment's margin of error. That is, says Parker, it's effectively zero.

Parker: It's as simple as that and that's the end of the story on it.

Baron: But Gene Mallove hasn't let the story end there. He has continued to press Parker and his colleagues for their lab notebooks to check their work. Parker has declined to turn them over, saying to collect all the relevant papers would be time consuming and not worth the effort. One MIT faculty member familiar with but not involved in the dispute said he thinks Mallove's claims have a germ of truth to them. The professor, who asked not to be identified, said scientists at the Plasma Fusion Center didn't keep a very open mind to the claims of cold fusion. The faculty member said he doesn't think anyone was guilty of deliberate misconduct, though they may have conducted bad science. MIT's administration has so far stayed out of this cold fusion controversy. Gene Mallove hasn't pushed for any formal inquiry, and the Institute's Provost, Mark Wrighton, says he doesn't see anything worth investigating. Observers say even if MIT scientists were guilty of misconduct, it's unlikely their actions changed the course of history. By the late spring of 1989, scientists across the country were coming to the same conclusion—that the Utah work was seriously flawed. And while intriguing claims of cold fusion continue to be reported at laboratories around the world, the field has had a hard time regaining its credibility. For WBUR, I'm David Baron.

Exhibit Q
Prof. Ronald Parker's Letter to Eugene Mallove
August 13, 1991

After the WBUR radio program, another fax arrived from Prof. Parker, this one having some but not all the requested data appended to it. And, he was asking me not to disseminate data that had been generated during a Federally funded research project!

Dr. Eugene F. Mallove, Bow, NH

Dear Gene:

At my request, Stan Luckhardt dug out the data you requested concerning the heater power for the H_2O cell in the Phase-II calorimetry measurements. For convenience in comparing with D_2O, he plotted the corresponding D_2O result on the same scale. The two graphs that he produced accompany this letter.

Since these data are unpublished, they are provided only for your information and should not be reproduced or disseminated without our permission.

Sincerely, Ronald R. Parker

XC: S. Luckhardt, K. Campbell

Exhibit R
Eugene Mallove's Formal Request for an Investigation of Scientific Misconduct at MIT

The stonewalling and obstruction of fact-finding about the MIT PFC experiment had been unrelenting. After much deliberation, I made the decision to submit a request for a formal investigation of scientific misconduct.—EFM

Dr. Eugene F. Mallove
The Writing Program, 14N-316, Department of Humanities
Massachusetts Institute of Technology, Cambridge, MA 02139

August 18, 1989

Dr. Mary P. Rowe
Special Assistant to the President, Room 10-213
Massachusetts Institute of Technology
Cambridge, MA 02139

Dear Dr. Rowe:

I regret to have to bring to your attention a serious matter of possible scientific misconduct, which has to do with the mishandling, analysis, and representation of scientific data by a group of researchers at MIT. Much as I did not wish to involve your office in a formal inquiry into this matter, I believe that the circumstances and evolution of this controversy now demand this action.

Specifically, I refer to some key data presented in a technical report of July 1989 done by a team of researchers from the MIT Plasma Fusion Center and their associates in the Department of Chemistry. The report is designated PFC/JA-89-34, "Measurement and Analysis of Neutron and Gamma Ray Emission Rates, Other Fusion Products, and Power in Electrochemical Cells Having Pd Cathodes"(Attachment #1). This report was subsequently published as a paper in the *Journal of Fusion Energy*, Vol. 9, No. 2, 1990, pp. 133-148 (Attachment #2). The research was supported, the report says, "in part by the United States Department of Energy contract number DE-AC02-78ET51013." So I presume that not only MIT guidelines but Federal regulations as to the handling of scientific data govern here. The sixteen authors of the paper are: D. Albagli,[1] R. Ballinger,[2,3] V. Cammarata,[1] X. Chen, R. Crooks,[1] C. Fiore, M. Gaudreau, I. Hwang,[2,3] C.K. Li, P. Linsay, S. Luckhardt, R.R. Parker, R. Petrasso, M. Schloh,[1] K. Wenzel, and M. Wrighton[1] [1=Dept. of Chemistry, 2=Department of Nuclear Engineering, and 3=Department of Materials Science and Engineering].

As an engineer with a long-standing professional interest in energy systems and in my former role as chief science writer at the MIT News Office, my interest in this paper evolved as the controversy about the claims of cold fusion emerged beginning with the announcement on March 23, 1989 by Professors Fleischmann and Pons at the University of Utah. As you know, I am the author of a book, *Fire from Ice*, about the scientific, political, and media aspect of the cold fusion controversy. My recent resignation from the MIT News Office was prompted by my deep concerns about the way prominent members of the MIT faculty have dealt with the subject. However, the issue at hand is not

New Energy Times Archive

the truth or falsity of cold fusion claims, nor the issue of the way in which cold fusion has been regarded at MIT generally, but the manner in which a specific piece of scientific research was conducted by one particular group at MIT.

Yet the effects on the scientific process to understand a baffling new phenomenon cannot be ignored. The MIT work has been widely cited as a keystone in dismissing the claims of Fleischmann and Pons. It also no doubt was one factor in the decision by former Chemistry Department head, Mark S. Wrighton, to sign the DoE Energy Research and Advisory Board report (November, 1989), "Cold Fusion Research," which most observers would agree eventually ended DoE funding of cold fusion research in the United States. Professor Wrighton was a key author of the MIT research paper.

The MIT work in question has to do with calorimetry, the measurement of heat in electrochemical cells that were set up by the MIT research group to test the claims by Professors Pons and Fleischmann to have measured significant excess power in some cells. The part of the paper that is of most concern is the section on calorimetry, not the results of nuclear measurements, although I do believe that the paper's statement in its final summary with regard to nuclear measurements is completely unfounded: "Importantly, the level of fusion products present is by far a more sensitive indicator of fusion reactions than are the relatively insensitive heat-based measurements which form the foundation of the claim of nuclear fusion as put forth by FPH." This statement presumed that all the possible nuclear reaction paths and products that might explain cold fusion were known *a priori* and were measured by the MIT group, clearly a statement that an objective scientist investigating a puzzling new phenomenon should not be making. In fact, theoretical and experimental developments have, I believe, totally invalidated those early beliefs by the MIT team. So the nuclear effects part of the paper, while useful and interesting, does not go to the heart of the cold fusion question as it was in 1989: Is there unexplained excess power in some electrochemical cells run in heavy water and palladium-platinum electrodes?

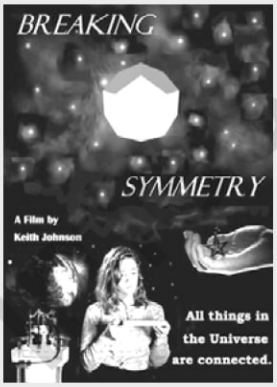
I respectfully ask you to initiate an immediate inquiry to determine whether a thorough investigation of the specific concerns that I have enumerated below is warranted. I believe that these concerns are certainly of sufficient magnitude to justify an investigation consistent with the Policies and Procedures guide for MIT faculty and staff. Those who are co-authors of the paper, including Professor Wrighton, should, of course, disqualify themselves from participating in the inquiry and subsequent investigation. I also request that Dean of Science Professor Robert Birgeneau not be involved in any inquiry and possible subsequent investigation, due to his well known negative view of cold fusion research.

The objectives of the investigation should be three-fold: (1) To determine whether there was misconduct in the handling and representation of the data to other scientists and to the public; (2) To determine whether there was misconduct and/or sufficiently egregious errors in the work to warrant the paper's retraction or significant amendment; and (3) To determine whether two of the paper's authors, Professor Ronald R. Parker and Associate Professor Ronald Ballinger, engaged in unethical behavior in orchestrating a public attack on the motives of researchers whose work they hoped to prove incorrect -- in part with the forementioned research paper. Further, to determine whether Professor Parker subsequently engaged in unethical behavior in deceiving the scientific community, the MIT News Office, and former MIT President Paul Gray about the nature of his actions.

Specific Concerns Enumerated

The MIT Plasma Fusion Center/Chemistry Department experimental contribution to cold fusion research was conducted from late March 1989, through late May 1989. The search for evidence of cold fusion in heavy water cells and comparisons with light water cells included attempts to find various nuclear products, as well as indications of excess power.

One experiment in the series of experimental cells reported in this work is of particular interest, because it is the only case in which some of the graphs of the raw data that form the power measurements are shown. This so-called "Phase II" calorimeter experiment compared the power production of a light water control cell and a heavy water cell. The record of the controversy clearly shows that at that time scientists were placing great emphasis on the need to find differences in the power production between light water cells and heavy water cells. Numerous statements by a variety of scientists at the time—on both sides of the controversy—attest to this, and these statements could readily be assembled if necessary. The presumption by many at the time and subsequently was that if a heavy water cell produced excess power and a light water cell of identical form did not, then there would be motiva-



BREAKING SYMMETRY

Cold Fusion Movie Imitates Life. . .

"COMING TO A THEATER NEAR YOU"

Written, Produced, and Directed by former MIT Professor Keith Johnson

(Dept. of Materials Science) [Excerpts from a Synopsis by Writer, Producer, and Director Keith Johnson]

Carolyn, late twenties, a recent Ph.D. in Astrophysics from Cal. Tech., where she developed a theory of "cosmic dark matter," arrives at a renowned Boston area Technical Institute to fill an Assistant Professor physics faculty position. There she meets her supervisor, Professor Klinger, a pompous physicist who heads up the Fusion Energy Lab devoted to harnessing the nuclear energy of the sun. Carolyn is told she is replacing another woman, Yvonne, who was denied tenure, and (under threat of future tenure denial) is discouraged by Klinger from continuing her own work on dark matter.

While moving into her apartment, Carolyn meets Steven, a consultant chemist, and the chemistry between them begins. Steven tells Carolyn he knew Yvonne because of his academic interest in Yvonne's controversial Institute research on "cold fusion," a cheap, environmentally safe way of generating energy from water. Steven informs Carolyn that Yvonne was denied tenure because her work on cold fusion threatened Klinger's costly "hot" fusion empire. . .

. . .The film reaches an explosive conclusion at a rehearsal for Carolyn's physics convention lecture. There we learn the shocking truths about. . .Klinger's role in suppressing cold fusion, and last but not least, the connection of the cold fusion energy to cosmic dark matter.

New Energy Times Archive

tion to investigate further the possibility that unknown nuclear reactions might be occurring—particularly if the magnitude of the integrated release of energy were to exceed the energy from any conceivable chemical reactions in the cell.

1. Shift of Intermediate Processed Data, Perhaps to Suppress Evidence of Possible Excess Power

This is one of the most troubling aspects of the Phase-II excess power experiments. Even if the processing of the raw data for this experiment were presumed to be correct, which I doubt to be the case (see subsequent listed concerns), data provided to me in the summer of 1989 by members of the MIT research team show that there was likely to have been an apparent difference in the performance of the heavy water cell and the light water cell; at least on one extant graph the heavy water water cell seems to be evolving excess power, while the light water cell seems not to be—exactly what many hoped to find at the time as an anomalous effect.

Attached are four figures, which have been adjusted in size by me from their original form to better compare them (see Attachment #3). The first two are from the published MIT paper and show the excess power produced by the Phase-II heavy water and light water cells. The intermediate processed signal was noisy and so was averaged in hour-long intervals to produce the data (black dots) seen in these figures. The results rise and fall above the zero excess power line, and there is nothing that leaps out from this data comparison to suggest that excess power is being produced in the heavy water cell and not in the light water cell. It looks as though both excess power plots are about equally noisy. This data that form these curves was prepared at least as early as July 13, 1989, because I was given a draft article by the PFC that bears that date (see Attachment #4). [Ed. Note: See graphs, p. 74.]

On the other hand, I also was given the processed but unaveraged , and hence more noisy data that went into forming these published curves. These data appear in the two other attached figures. The figures are copies of graphs that appear in another PFC draft report to me on calorimetry, dated July 10, 1989, three days before the draft with the averaged data (see Attachment #5). The light water graph oscillates above and below the zero excess power power line (which I have introduced to make the comparison clearer), with no obvious bias above or below the zero line. There appear to be cyclic variations in the presented excess power, but it is not clear what these are from. The heavy water curve, by contrast, is dominantly above the zero line, indicating the possibility of a residual excess power (even though the magnitude of the excess power may be below the report's stated sensitivity of their calorimetry, 40 milliwatts). The two curves are simply quite different. There could be something like a few tens of milliwatts excess power here, on average, as several PFC researchers have subsequently agreed there could be. For this 0.1 centimeter diameter electrode, 9 cm long, 20 milliwatts would translate to excess power of 0.28 watts per cubic centimeter.

So why do we see no evidence of this possible excess power in the graphs that are in the final report and the published paper? The inescapable answer seems to be that the averaged data for the heavy water was moved down an arbitrary amount so that it now has more the appearance of the null result in the case of the light water averaged data. Interestingly, the light water averaged data seem to be consistent in level with the corresponding curve of raw processed data, that is, it has not been moved down.

In a recent letter to me (see Attachment #6), Professor Ronald R. Parker, who has been aware of my concerns since at least early June of this year, states: "Our paper estimates the uncertainty of calorimetry measurements as 40 mW, and so you are free to posit an excess heat less than this level if you wish." This blithe remark is of concern for a number of reasons. Foremost is because I reject the entire methodology of getting to these intermediate processed curves in the first place (see below). Second, there is no substantiation of how this 40mW sensitivity of the calorimeter was derived—certainly of key importance in knowing whether any significant shift of the data is warranted at all. I requested via a letter of April 29, 1991 to another team member, Dr. Stanley Luckhardt, information on how the sensitivity of the calorimeter was computed (see Attachment #7). No information on this point has been forthcoming from him, as you will see below.

There is yet another concern about the downward shift in the heavy water cell data. Note that on page 4 of the July 10 draft of the calorimetry section (Attachment #5), there is no final paragraph of conclusions. However, on the July 13 draft (Attachment 4), a conclusion paragraph has been inserted—after the curve was shifted down. The key sentence is: "The data show a slowly fluctuating power level in both the H_2O and D_2O cells, but neither show evidence of sustained power production at the levels claimed in Ref. [Pons and Fleischmann's report]." How is one to judge what level of power may have been present in the heavy water case if the heavy water curve is arbitrarily moved to make it look like the light water curve? The readers of this report are expected to believe that the calorimetry was sensitive enough to present evidence of a "slowly fluctuating power level" (clearly with less than 40 mW sensitivity required to observe them), but they are misled by the contrived down-shift of the heavy water power curve.

2. A Grossly Incorrect Analysis of Raw Data that was Guaranteed to Produce Zero or Minimal Difference Between Light Water and Heavy Water Power

There is a simple equation cited by the paper's authors that governs whether some unknown source of anomalous power, p_x, is acting in the cell. That equation is: $p_h + p_x$ = constant. Now by the methodology of this paper, if the heater power, p_h, is declining in time, that would be an immediate indication that there might be some excess power building up. Now if one fits any kind of curve to the ragged raw data that describes p_h as a function of time and then subtracts this from the raw data, one does not get p_x. Yet this is precisely what the group apparently did. The result is merely a reflection of the "noise" that is left over once the subtraction is done. There is no way that one can say that this processing gives p_x.

It is obvious that p_x is linearly related to p_h, but to justify a subtraction of any part of the raw p_h signal would require a careful thermal analysis of the cell's heat transfer characteristics to determine how much, if any, of the heater power drift should be removed. This analysis was not done by the MIT group.

There has been a more elaborate technical analysis of this experiment, which leads me to believe that at least in the heavy water case in the Phase II experiment, there could be evidence of excess power production. The evidence of excess power production based on this analysis appears in a technical letter to the editor in *Fusion Technology* , Vol. 19, May 1991, pp. 579-580 (see Attachment #8). In that letter, the power density produced in the palladium electrode is shown to rise from zero at 20 hours into the test to close to 2 watts per cubic centimeter at 100 hours. In view of the very belated release of the light water heater power curve (see concern 6. below), I am less confident of all the details of the analysis in *Fusion Technology*, but I am very confident that the MIT group's method is without justification.

The analysis on which Drs. Noninskis' conclusion in *Fusion Technology* is based differs from the MIT group analysis because the latter analysis has introduced the forementioned subtraction from the raw data that does not appear to be proper -- a subtraction from the raw data of a linear fit to the noisy and declining heater power. In fact, if this adjustment is performed, for the reasons suggested in the MIT paper, it is possible to get nearly a null result for the excess power measurement in every case. It is surprising that this was done, because in the methodology of the MIT calorimeter, declining heater power should have suggested the presence of an unknown heat source. In Professor Parker's letter to me of August 8, 1991 (Attachment #6), remarkably he acknowledges that the subtracting out of p_h due to the depletion of the electrolyte may not necessarily be justified : "Whether this is a correct assumption is arguable. . ." Then he states by pure fiat: "but in any event, the main conclusions stand: We detected no significant difference between H_2O and D_2O. . ." In other words, even though the PFC's assumptions about the manipulation of the data may be incorrect, we are expected to believe that there is no significant difference between H_2O and D_2O because the PFC group says without substantiation that its calorimeter's sensitivity was 40 milliwatts.

3. False Assertion About the Significance of Unpublished Test Results That Motivated The Incorrect Analysis

The authors of the MIT work attempt to justify their unwarranted subtracting out of the heater power. On page 20 of the PFC report (Attachment #1) we find the statements: "However, measurement of p_h over a 100 h period, Figure 6, indicates a significant drift caused by a reduction of the solvent volume. We demonstrated that this drift was

New Energy Times Archive

due to solvent loss rather than to an unknown power source, p_x, by calibrating ph as a function of the electrolyte solution volume. When enough solvent was added to the D_2O cell to compensate for that lost to the electrolysis at the end of the 100 h period shown in figure 6, p_h returned to within 20% of its original value." This statement is shocking because the authors are suggesting that a 20% discrepancy in heater power (over 200 milliwatts) to heat the same volume of fluid that was present initially can be ignored. In fact, if this experimental finding of a 20% discrepancy is true, it might be the best evidence of all that the heavy water cell really did produce anomalous excess heat.

The report immediately follows with yet another blatantly untenable statement: "If the total volume of solvent loss over the course of the experiment had been taken into account, including that lost to evaporation, p_h would have been even closer to its original value." If the cell has been refilled to compensate for the water lost to electrolysis, then what fraction of that refilled water disappeared through evaporation and what fraction disappeared through gas evolution is irrelevant. The net effect of these statements—just as in the unexplained downshift—is to obscure the possible evidence for excess power.

4. Subsequent Claims that the Basic Objective of the Research was Other than Claimed in the Report

Long before the concerns that are being enumerated in this letter had been brought to the attention of the MIT group, I met with Dr. Stanley Luckhardt to try to understand in detail how the MIT work was done. Frankly, I was politely looking for assurances that the data had not been arbitrarily down-shifted and that there might be some fundamental unpublished reason for the shift. I met with Dr. Luckhardt on January 25, 1991 and, without being accusatory at the time, received extremely unsatisfactory explanations. Dr. Luckhardt could not explain how the "bias," as he called it, was taken out to make the resulting power curves seem so similar. In fact, he agreed at the time that there might be 20 milliwatts of excess power in the MIT group's heavy water cell, "but not the 80 milliwatts that Fleischmann was talking about."

This litany continues of redefining the objectives of the Phase II experiment to argue that no unwarranted data moving has occurred. In the belated letter of explanation that I received from Professor Parker (see Attachment #6) he writes: "The implicit assumption is that we were looking for fast turn-on of the anomalous heat production and so it was legitimate to subtract out a slow baseline drift caused by depletion of the electrolyte." It seems to me that technical papers cannot have "implicit assumptions" as to their basic objectives. The basic objective was to find out whether there was a difference between heavy water and light water performance. There is no clearer evidence that such a comparison was what was originally intended by the MIT group than the paper's remark about the comparison of the light water and heavy water cell power output near the end of a 200 hour run (page 19 of Attachment #1). There is no discussion there of looking for "fast turn on" of power. I believe that this is a newly invented experimental objective to justify any past juggling of data that the group might have done.

5. Public Renunciation in June 1991 by Professor Parker of the 1989 Calorimetry Work, Whereas the Work Was and Continues to be Promoted as Sound and Definitive Regarding Excess Heat

On June 7, 1991, during the question and answer period following the talk by Dr. Frank Close at the PFC, Professor Parker had this exchange with me (transcribed from a tape of the meeting):

Parker: We at MIT looked very carefully at Fleischmann and Pons, and this is what we came up with. [If we] think we ought to look at another set of experiments and we think we have expertise, we will. But just let it fall where it lies. We're not going to come out one way or another until we look at it.

Mallove: Would you consider re-evaluating your own experiment, if I brought in experts to evaluate it? Would you consider that? Because I've asked Dr. Luckhardt for several weeks now—and I know he's not here today. He told me at one point he would provide me with the heater power curve for the light water experiment so that I could ascertain what the heck was going on in that experiment. He then finally ended up saying to me he would not give it to me—or that it would take a week to do it.

Parker: I think, Gene, that what you showed up here earlier is completely a surprise to me. [The Phase II comparison power tests of light water versus heavy water, published and unpublished versions.] We will give you every piece of data we ever took.

Parker: My personal. . .

Mallove: Fine.

Parker: I'll tell you what my opinion is of that work, because I was part of it. I don't think it's worth very much. Alright? And that's why it's just published in a tech report. I don't think it's worth very much. I think to do calorimetry is one of the hardest things I ever tried to do. I'd rather stick to plasma physics.

Mallove: But, Ron, with all due respect, I agree with you, I agree with you [that the work was not conclusive].

Parker: When you have an open system is where you can make big errors, where you don't know the overpotential, the electrode potential, and so on. These things are unknown. I mean it's really tough and that's why I don't put any stock at all -- you can redraw those curves anyway that you want. I don't think that data is worth anything. Now you may be able to find something in it. I did the experiment; I don't think it's physics.

Mallove: But what I've seen, because I certainly see it from Douglas Morrison [of CERN] and I see it from people like Frank Close and others, that your prestigious laboratory with its excellent resources is being used in some respect as a standard which everyone else is supposed to adhere to. . .

Professor Parker continues to insist in representing his data in two different ways. Both before and after this remarkable June 7, 1991 renunciation we have statements that the work shows no evidence of excess power. How is it that Professor Parker thinks he can disparage his group's own work in public and later claim that it has validity? This is not a small point. The MIT work has been cited on numerous occasions by people bent on attacking cold fusion researchers for their supposed "delusions" and "incompetence." We never before heard from any member of the PFC team that the work was "worthless." Quite the opposite. Professor Parker's colleague Professor Mark S. Wrighton in a letter on October 10, 1990 (to Dr. Vesco Noninski (see Attachment #9), was quite firm that the MIT work was definitive.

6. Obstruction of Other Scientists' Attempts to Access Data Critical to Understanding the Group's Experiments

It is clear that for about a year now the MIT group has been very reluctant to release data that would allow other researchers, including myself, to gain insight into their calorimetry work. I introduced an electrochemist visiting from Bulgaria, Dr. Vesco Noninski, to Dr. Stanley Luckhardt on August 15, 1990. Noninski had viewed the MIT work very positively, because it was one of the few experiments in calorimetry in which at least some raw data was published. He had begun to believe that the MIT heater power data for the heavy water cell could be analyzed to show evidence of excess power. There followed discussions between Drs. Noninski and Dr. Luckhardt in which it became clear that there was a difference of opinion between the two on how the experiment should be analyzed. But in spite of Dr. Noninski's completely valid request to Dr. Luckhardt to receive the raw data corresponding to the light water cell, he would not release this data. Both Dr. Luckhardt and Professor Wrighton wrote letters to Dr. Noninski in which they dismissed his analysis outright.

Much later, in early May 1991, I was attempting to write a paper for *Fusion Technology*, to assess the MIT Phase-II calorimetry and to remark about the relative points made by Drs. Noninski and Luckhardt. So I asked Dr. Luckhardt if he could give me the light water heater power curve. Initially he said he would do this at a pre-arranged meeting between us. There followed over the next several weeks a series meetings that were cancelled by him. Finally on May 29, 1991, in a lengthy phone conversation with Dr. Luckhardt, he said that he would not turn over the data to me. The reasons he offered were that it would take too much time for him to explain to me how to correctly interpret the data. He said it would take four or five days to interpret the data. He further put me off by suggesting that I talk to another group "perhaps SRI" to get advice on calorimetry. Even though he would not give me the data, he said of the two heater power curves that they "looked pretty much the same" but that "fluctuations" were different, and he could not recall which one was unusual in which respect.

After I showed the apparently down-shifted intermediate processed data at the seminar at the Plasma Fusion Center on June 7, 1991, Professor Parker publicly said, "We will give you every piece of

New Energy Times Archive

data we ever took." So on June 14, 1991 I submitted a request to Professor Parker for various data items that would help me clarify the issue (Attachment #10). Another scientific colleague, Dr. Mitchell R. Swartz, independently had made several phone calls to Professor Parker, in which messages were left about requests for the data that Swartz had heard Parker on June 7 offer to make available.

Not having received any communication from Parker regarding my June 14 request, I submitted a reminder letter on July 30, 1991 (Attachment #11). There was no response to this letter either. Nor were any of Dr. Swartz's calls returned by Professor Parker. The first response from Professor Parker came in a faxed letter to me (Attachment #6) on the evening before a WBUR radio program about cold fusion, in which Parker and I were both pre-recorded participants. In this letter, he continued to put obstacles in the way of getting the light water heater power curve. None of the other data requests of June 14, 1991 were even alluded to. In this letter he claims that Dr. Luckhardt "has been extremely forthcoming in discussing them with you and would be willing to assist you further." He wrote, "However, he has major responsibility for several PFC projects and I cannot allow him to take time away from them for this purpose. I suggest that you negotiate directly with him to see what arrangements could be made."

Finally, to cap this absurd one-year history of evasion at every turn, I received yet another fax letter from Professor Parker on August 13 (Attachment #12), in which inexplicably there has been some time for Dr. Luckhardt to generate (or simply retrieve from his files) the light water heater power curve. But the letter suggests that the accompanying curves (both heavy water and light water) "should not be reproduced or disseminated without our permission." This statement is put forth even though the heavy water heater power curve was already published by the MIT group! For reasons unknown to me, Professor Parker wants the light water power curve to remain out of the public domain.

I think that it is absolutely clear that there has been an unwarranted withholding of data by the MIT group from people with a legitimate need to access it. In fact, this withholding of data was the turning point that prompted this request for an inquiry. I had considered making the request earlier, but was very reluctant to make it. However, the final evasions raised even greater suspicions in my mind about the nature of this data and how it was handled.

7. Attacks on the Motives of Scientists Who Were Making Positive Claims About Cold Fusion

The method of performance, representation, and access to the MIT team's research is the central issue of this request for an inquiry and investigation. However, the MIT group's work did not occur in an intellectual vacuum. It is impossible to dissociate from the MIT group's performance the unethical behavior of some of the key organizers of that research in view of their attacks on the motives of Professors Pons and Fleischmann, whose work they apparently wished to discredit with their own results. This raises questions about the integrity with which the MIT group's work was carried out.

On May 1, 1989, the MIT News Office issued a press release (see Attachment #13) that was prepared by me in telephone consultation with Professor Parker of the Plasma Fusion Center in the early morning hours of May 1, 1989. The press release was crafted to deny statements that *Boston Herald* reporter Nick Tate had attributed to Professor Parker, which attacked Professors Pons and Fleischmann in the manner denied in the press release. When I prepared that release, and for more than a year afterward, I believed that the statement that the MIT News Office had issued was valid, though I had some doubts. I simply trusted what was being asked of me in preparing the release.

After having listened to the actual tape of the interview that Nick Tate played for me in July 1990, I am certain that his story's characterization of what Professor Parker and Professor Ballinger had said in their conversation with Tate the week before is substantially correct. Professor Parker and Ballinger clearly were conducting a well-orchestrated attempt to condemn the work of Pons and Fleischmann, not merely to criticize it technically. In the taped interview, a copy of which I obtained from Nick Tate in February 1991 (a partial transcript of which appears in Attachment #14), Professor Parker used the word "fraud" no less than five times and he does refer to the work of Pons and Fleischmann as "scientific schlock," despite his subsequent and continuing denials. At one point in the interview Professor Parker says to Nick Tate, "..what you're hearing is that we think its a scam, right?" In my view, virtually any other competent reporter would have written essentially the same story that Tate wrote. In fact, the partial transcript published by the Herald on May 2, 1989, should have been enough to convince me of the soundness of Tate's story, had I not been told by Professor Parker that these remarks were "out of context," and that reporter Tate was a "viper."

Professor Parker deceived me and other members of the MIT News Office about what he had said, then in a press conference on May 1, 1989 he deceived the world about what he had said, and he continues to deceive the world again and again (see transcript of recent WBUR radio program, Attachment #15).

Remarkably, Professor Parker apparently deceived even former MIT President Paul Gray about the nature of his effort to manipulate the News Media. An April 17, 1989 letter from *Boston Globe* Reporter Richard Saltus to President Gray (see Attachment #16) had complained about the lack of access to Professor Parker. President Gray's letter of response to Richard Saltus on May 1, 1989 (Attachment #17) says, "He [Parker] has tried to be as helpful as possible, consistent with his belief that judgement should be reserved until the scientific facts are clarified. That cautious stance has led him to discourage all media visits to the Plasma Fusion Center, although his efforts have not always been successful. I have been assured that there was no discrimination against the *Boston Globe* and that, to the contrary, Professor Parker spoke five or six times with your colleague, Mr. David Chandler."

The truth about Professor Parker's media manipulation is clear from this interview with Nick Tate. At one point Parker says, "The reason I stopped talking to the *Globe* for example is that I felt that they were reporting irresponsibly...they were out there just leading the cheers instead of being objective." Then later, ". . .you know I can't trust the *Globe*, I'd like to trust you."

This deception and compounded deception about what was said and how it was arranged to be said to Nick Tate amounted to a direct attack on the integrity of an honest reporter as well as the contrived involvement of a number of MIT personnel, including the former President, in that deception. But as important or more so was the effect that the intended disparagement of Professor Pons and Fleischmann's work had on the future course of cold fusion research. The work was difficult enough to assess without imputing the motive of possible fraud, which allegation at that time and to the present day remains completely unfounded.

Professor Parker was not the only member of the MIT research team to bring up allegations of fraud about Professors Pons and Fleischmann. I am aware of at least two other authors of the MIT research paper who have made such allegations publicly and privately.

I hope you will determine that the concerns I have raised about the conduct of the MIT group's work merit a prompt investigation. If carried out objectively, such an investigation will, I am very confident, reveal various levels of scientific misconduct.

To conclude this formal request, I would like to make clear my belief that however the data from the Phase-II calorimetry experiment are interpreted, it is clear that it is impossible to use the raw data behind these flawed results or the data of the cruder series discussed earlier in the PFC paper to conclusively prove anything one way or another about the reality of cold fusion. I do not believe that any member of the MIT research group ever actually believed it had discovered evidence for cold fusion and then "covered it up." However, I do firmly believe that they mishandled their data in a manner calculated not to leave any room for doubt about the finality of their conclusions, when in fact there was considerable room for such doubt. In my view, the data in this work were not only improperly manipulated to emphasize an allegedly negative result, but the significance of conclusions about that data were and continue to be misrepresented and confused.

Even if this work had been done properly and offered a clear null result, it would still have been far too limited a check. Other laboratories that have obtained sporadic positive results for excess power—and by now even reproducible excess power (SRI, Inc. in Palo Alto under EPRI contract)—have generally spent much more time in their trials and employed a larger series of electrodes, a possible requirement sometimes to pick up the anomalous thermal effects. The body of evidence in support of some if not all cold fusion claims, in fact continues to grow, as can be seen in several reviews and compilations of many experiments:

* Edmund Storms (Los Alamos National Laboratory), "Review of

New Energy Times Archive

Experimental Observations About the Cold Fusion Effect," accepted for publication in *Fusion Technology*, 1991.

* M. Srinivasan (Bhabha Atomic Research Centre), "Nuclear Fusion in an Atomic Lattice: Update on the International Status of Cold Fusion Research," *Current Science*, 25 April 1991.

* Steven E. Jones, Franco Scaramuzzi, and David Worledge (editors), Anomalous Nuclear Effects in Deuterium/Solid Systems, American Institute of Physics Conference Proceedings 228, 1991.

* *Investigation of Cold Fusion Phenomena in Deuterated Metals* (four volumes), by the National Cold Fusion Institute, June 1991, now available from NTIS.

What is most disturbing, however, about the way in which the MIT research was conducted was the arrogant dismissal of the work of other scientists with the group's insubstantial, manipulated, and flawed evidence. It smacks of a rush to judgement by a group that had made up its mind that it would never find anything positive nor would it report such. Even before they had completed their experiments, members of the MIT group made it quite clear to the world in so many ways that they didn't expect to find anything. The parallel effort to discredit the motives of other scientists and the denial that such an effort was ever made has had a lasting corrosive effect on the entire field. I hope that when the inquiry and expected investigation of this matter is completed, MIT will adopt guidelines to insure that this behavior is never repeated.

Sincerely, Eugene F. Mallove

List of Attachments (those with * can be found in this issue)

1. Plasma Fusion Center Report, PFC/JA-89-34, "Measurement and Analysis of Neutron and Gamma Ray Emission Rates, Other Fusion Products, and Power in Electrochemical Cells Having Pd Cathodes"
2. The corresponding paper in the *Journal of Fusion Energy*, Vol.9, No.2, 1990, pp.133-148.
***3.** Four figures, two of which were obtained from the MIT team's draft material and two of which were published. These have been adjusted in size by me from their original form for easier comparison.
4. Draft report of calorimetry section, July 13, 1989.
5. Draft report of calorimetry section, July 10, 1989.
***6.** Letter from Professor Ronald R. Parker to Eugene Mallove, August 8, 1991.
***7.** Letter to Dr. Stanley C. Luckhardt from Eugene Mallove, April 29, 1991.
8. A technical letter to the editor in *Fusion Technology*, Vol. 19, May 1991, pp. 579-580.
***9.** Letter to Dr. Vesco C. Noninski from Professor Mark S. Wrighton, October 10, 1990.
***10.** Letter (June 14, 1991) from Eugene Mallove to Professor Ronald R. Parker, requesting data that was promised at an open public meeting.
***11.** Letter to Professor Parker from Eugene Mallove, July 30, 1991.
***12.** Letter to Eugene Mallove from Professor Parker, August 13, 1991.
***13.** MIT News Office press release, May 1, 1989.
***14.** Partial transcript of April 28, 1989 interview by *Boston Herald* reporter Nick Tate with Professor Ronald R. Parker and Associate Professor Ronald Ballinger.
***15.** Transcript of David Baron's WBUR radio program (August 9, 1991) about the cold fusion controversy.
***16.** Letter from Richard Saltus of the *Boston Globe* (April 17, 1989) to President Paul E. Gray.
***17.** Letter from President Paul E. Gray to Richard Saltus (May 1, 1989).

Exhibit S
Permission Given to Transmit Request to Dr. Vest
September 9, 1991

It was necessary for Dr. Rowe to have my formal permission to submit my request for an investigation to President Vest.—EFM

Dr. Eugene F. Mallove
The Writing program, 14N-316, Department of Humanities
Massachusetts Institute of Technology

Dr. Mary P. Rowe
Special Assistant to the President, Room 10-213
Massachusetts Institute of Technology

Dear Dr. Rowe:

As per our telephone conversation of last week and earlier telephone message exchanges, I agree with you that my formal request for an inquiry and investigation of possible scientific misconduct at MIT (letter and attachments dated August 18, 1991) should be redirected by your office to President Vest. Though I am assuming that you have already made this transfer, I hereby formally give you permission to direct the request to Dr. Vest.

Sincerely, Eugene F. Mallove

Exhibit T
Eugene Mallove's Response to a Statement on Cold Fusion Issued by the MIT News Office 8/30/91
Received by Mallove only on 9/16/91

The MIT PFC had put out an outrageous "Press Release" on August 30, 1991, which I did not get wind of until mid-September. Here is my rebuttal of its various claims. —EFM

Dr. Eugene F. Mallove
Bow, New Hampshire, September 17, 1991

On August 30, 1991, the MIT News Office issued a disgraceful and misleading statement about the cold fusion controversy (see attached). What follows is my point-by-point response to this statement. Bold type are quotations from the August 30, 1991 MIT News Office statement.

• **"MIT scientists intensely investigated the phenomenon called 'cold fusion' for two months in 1989. Like other scientists around the world, they were unable to duplicate the Pons and Fleischmann experiment They have concluded that, while the reaction termed cold fusion is scientifically interesting, it is not one which is valuable for them to pursue at this time."**

Note the implication that all scientists at MIT who had anything to do with cold fusion investigated it for only two months and then dropped it after being unable to duplicate the Pons and Fleischmann experiment. This is not true. To my knowledge, at least five MIT professors continue to be actively interested in cold fusion experiments and theories. Numerous other professors, researchers, and students at MIT, not directly engaged in cold fusion, have expressed intense curiosity about the status of current cold fusion research. At least two MIT professors have applied for patents on cold fusion concepts. The cold fusion theory of one MIT professor is highly regarded in the field. Moreover, cold fusion research is right now being actively pursued at dozens of laboratories in the U.S. and abroad.

I challenge the term "intensely investigated for two months." The MIT professors who influenced this statement arrogantly suggest that it is possible to perform two months of experiments and come to definitive conclusions about so difficult a phenomenon as cold fusion. They are wrong about this and they know it, as the phrase "scientifically interesting" so clearly reveals. The preparers of this statement are well aware that they do not understand this "scientifically interesting reaction," nor is it likely that they have they kept current on what is really happening in this field, yet they claim to be able to determine that "it is not one which is valuable for them to pursue at this time." This is an indictment of the appalling lack of scientific curiosity manifested by these individuals. If this is supposed to be an example to MIT students about how deeply scientific curiosity should be followed, the Institute is in grave trouble.

The statement also incorrectly implies that other scientists have not been able to duplicate the Pons Fleischmann experiment. The phrase used was, "like other scientists around the world." Again, the statement preparers are engaging in smokescreen tactics. Numerous other investigators have to their own satisfaction and to the satisfaction of other objective evaluators replicated cold fusion phenomena, including excess energy evolution in excess of what can be explained by conventionally understood chemical or mechanical energy release, the production of tritium, the production of neutron bursts, low-level neutron emissions, and charged particle emissions.

• **"They note that the University of Utah, where 'cold fusion' began, has closed its cold fusion institute."**

What a pathetic bit of innuendo—to imply that scientific,

New Energy Times Archive

budgetary and programmatic difficulties at one research laboratory somehow validates the statement preparers' lack of interest in cold fusion research. For less than $5 million, the National Cold Fusion Institute generated a large body of impressive scientific data in a research program that was favorably critiqued by four outside scientists in the spring of 1991, some of whom were and continue to be skeptical of cold fusion. However, the reason that NCFI was mothballed had to due only with the inability to attract private funding for the Institute after state funding expired, as was the agreement from the outset. Since when do funding successes or failures reflect on the scientific and technological merits of a new area of research—as implied by the News Office statement?

• "He [Eugene Mallove] says that MIT scientists should be conducting further investigations into cold fusion. The question he raises is basically a matter of who should set MIT's research priorities. It is the role of the individual MIT professor to set those priorities. Research into this phenomenon is low on their priority list at this point."

Indeed, I have suggested to the President of MIT that research into cold fusion should be re-evaluated in view of mounting laboratory evidence. My letter of April 12, 1991, in which this opinion was brought to Dr. Vest's attention was never answered and remains unanswered to this day. I believe that it is the responsibility of the administration of MIT to be watchful for new scientific opportunities and to encourage, not enforce, various research directions. MIT presidents have advocated increased efforts in nuclear reactor safety, scientific literacy, molecular biological research, and the like.

Again we see an attempt to lump all MIT professors into the category of those who put a low priority on cold fusion research.

While drawing attention to the issue of research priorities, the preparers of the News Office statement have neglected to mention that in a letter dated August 18, 1991, I have requested a formal investigation of possible scientific misconduct on the part of some MIT researchers in their handling and representation of scientific data in their spring of 1989 cold fusion experiments. I have also asked for an investigation into the likelihood that certain researchers at MIT orchestrated an unethical attack on the work of Pons and Fleischmann. My concerns about cold fusion research at MIT are more comprehensive than matters of research priorities.

• "MIT scientists have reviewed their paper that contains the data about which Mallove raised questions. Following the review, Professor Ronald R. Parker said, the conclusions of the study stand as published."

The latter statement is completely untenable in view of the June 7, 1991 statement made by Professor Ronald R. Parker at an open seminar at the Plasma Fusion Center. On this occasion (when he no doubt thought no one outside the room would ever hear his words), Professor Parker severely deprecated the experiment that I have questioned, but now he wants the world to believe that it was a good experiment and that his group's negative conclusions stand. This is, in part, what Professor Parker said on June 7, 1991 about the experiment for which I have requested an investigation:

"I'll tell you what my opinion is of that work, because I was part of it. I don't think it's worth very much. Alright? And that's why it's just published in a tech report.*

" I don't think it's worth very much. I think to do calorimetry is one of the hardest things I ever tried to do. I'd rather stick to plasma physics. . . When you have an open system is where you can make big errors, where you don't know the overpotential, the electrode potential, and so on. These things are unknown. I mean it's really tough and that's why I don't put any stock at all—you can redraw those curves anyway that you want. I don't think that data is worth anything. Now you may be able to find something in it. I did the experiment; I don't think it's physics."

[*The research was published not only in a "tech report," but also in the *Journal of Fusion Energy*.]

Exhibit U
President Charles Vest's Letter to Prof. Morrison
October 9, 1991

CHARLES M. VEST, PRESIDENT, ROOM 3-208

In President Vest's request to Prof. Morrison to assess my request for a full investigation, there were already some disquieting signs. He was deferring consideration of the MIT PFC's "ulterior purposes." The press deception, as it turned out, was never addressed after an opinion of Vest's "legal counsel."—EFM.

Dr. Philip Morrison
Institute Professor Emeritus, Rm 6-205

Dear Philip:

Dr. Eugene F. Mallove has written to my office to bring to my attention "a serious matter of possible scientific misconduct, which has to do with the mishandling, analysis, and representation of scientific data by a group of researchers at MIT." Our normal procedure would be to turn to the Provost to make judgment whether and how to proceed to consider a matter of this type. In the present instance, however, the Provost is one of a number of authors of a paper referred to in Dr. Mallove's statement of concern. Therefore, I am turning to you, as a distinguished member of the community, to assist the Institute in formulating the initial judgments and/or actions that should be taken in response to Dr. Mallove's letter.

I am enclosing Dr. Mallove's letter, dated August 18, 1989 and a subsequent letter dated September 9, 1991. Both of these letters are addressed to Dr. Mary P. Rowe, and the second one asks that the initial letter be redirected by her to me. All background materials that were supplied by Dr. Mallove, together with his August 18 letter, are also enclosed.

I would ask that you review Dr. Mallove's letter and the attached materials, and give me your specific advice as to how the Institute should proceed. I would suggest that there are three options that you might consider:

1. Should an inquiry be conducted, and, if so, what mechanisms and individuals might serve this function well?

2. Should a formal investigation occur at this time, and, if so, what mechanisms and individuals might be appropriate?

3. Is there an alternative course of action to 1 or 2 above, which you believe is preferable?

It seems appropriate to me that consideration, at this stage, should focus on the criticisms of the science and methodology which are raised in Dr. Mallove's letter and not on the questions of motives or ulterior purposes attributed by Dr. Mallove to various MIT scientists. In my view, these questions are not appropriate to consider until after the scientific issues are addressed.

I very much appreciate your willingness to carry out this important task.

Sincerely yours, Charles M. Vest

CMV: cbb Enclosures
cc: Dr. Mary Rowe
bcc: Constantine Simonides
Mark Wrighton

New Energy Times Archive

Exhibit V
Prof. Morrison's Report to President Charles Vest
October 14, 1991

Professor Morrison's conclusion that the MIT PFC paper "does not mislead" is completely untenable. It does not square with the facts and it never will.—EFM

MIT Department of Physics, Room 6-205

To: Charles Vest, President
From: Philip Morrison, Institute Professor (emeritus)

Assessing the Request for an Inquiry by Dr. Eugene Mallove, made August 18, 1991

I. My Standing in the Matter

In the presence of clear concern for equity in treatment, I begin by stating my own qualifications and limitations as an assessor.

A theorist with considerable experimental experience--though to be sure, most of it gained long ago—I think I am qualified as a referee of the overall methods and content of a paper on cold fusion, denoted Plasma Fusion Center PFC/JA-89-34, prepared and published by an MIT team from PFC and three other MIT departments, with sixteen co-authors (*Journal of Fusion Energy*, 9, 133-148, 1990). It is this paper that is the focus of the substantive issues here, issues that grew in interest well after the summer of 1990.

I shall not comment on the relationships between PFC research people and the media during the heated summer of 1989, though that is also part of the request for inquiry. Others may consider them.

The topic crosses disciplines; indeed, that is one of the sources of trouble. My background in nuclear physics is strong, but I am not an electrochemist. Much of my general grasp of that specialty has been gathered during the cold fusion controversy itself within the last two years. I regard my task as that of an interested scientific reader, but not one who could propose detailed improvements in the entire experiment design. The editors who found referees for the paper would probably seek more than one referee. That understood, I shall support my opinions with inferences drawn from the paper and the criticism of it offered in Dr. Mallove's submission.

I know several of the participants in this dispute, none of them better than I know Dr. Mallove himself. His astronomical specialization and interest in science education and science journalism have brought us together over perhaps a decade, before and during his stay at MIT. Dr. R. Petrasso of the PFC, a nuclear experimenter, is the second person involved whom I know rather well. Our interaction extends over a few years, about a year longer than the cold fusion era. Of course I know the Provost and other more senior members of the team, but not with so much contact as with Mallove and Petrasso. I do not think I hold any sort of animus for any of these colleagues.

Toward the idea of cold fusion itself I was rather more tolerant and optimistic than most physicists. I still believe that there may be a germ of novelty in some electrochemical phenomenon that is caught up in this complex system; it is very unlikely, though logically possible, that new findings, if established, would turn out to have high economic importance. They would at most open some way to build a new battery, possibly a fuel cell.

II. The Substantive Issues

The papers before me are somewhat tangled. The core of the topic is the publication in the JFE. It is here, at least in part, in four distinct versions: the published paper, the MS finally submitted to the JFE and dated July 1990, and two partial drafts of that MS, both by S. Luckhardt of PFC, with signs of much comment by co-authors. The drafts are dated July 10 and July 13, 1989.

With 16 authors, no paper will have a simple history. This one shows that to be true; the published version itself is not the same as the MS submitted, but bears strong mark of editorial changes, in text and in figures. All of this is entirely to be expected.

Two distinct experiments are reported. Phase I was "hastily assembled," within days after the initial TV show from Utah. It sought both real-time nuclear radiation and excess electrochemical power. Phase II was more ambitious, designed as a much more sensitive test of all Utah claims by rough replication, and extended over a couple of months.

All MIT results were negative: real-time radiations, atomic products of fusion within the cells, and excess electrochemical power. In addition, a telling technical criticism of the published Utah-gamma-ray data is included. I could find no claim by the MIT authors that was not well supported by the data they include.

III. The Ground for Complaint

What then is the ground of complaint? It is not without a logical basis. The sensitivity of the MIT tests for nuclear radiations is improved by two or three orders of magnitude over the Utah experiments. Even stronger limits follow from the search for certain fusion product atoms. In mid-1989 the Utah [people] claimed such products in ample amount, easy to find. For the most part by 1991 those side effects were no longer claimed, and the main evidence cited was excess power during electrolysis of heavy water. It is perceived that a new form of fusion might escape all side branches to deliver energy as lattice heat alone, making helium as the only product. Even this seems limited by this experiment to a power down from the Utah claim by a thousand or so.

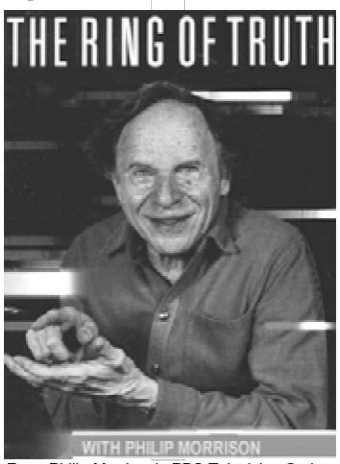
But the Utah investigators are electrochemists, and skilled in calorimetry. In that domain—if not in gamma-ray or neutron detection—they worked closer to the state of the art. The MIT team claimed an intrinsic sensitivity of their own calorimetry no better than 10 mW, from the noise visible in all their power measurements. They claimed overall only a conservative 40 milliwatt sensitivity, about 3% of total power, allowing for cumulative systematic changes as the open cells ran for a week or more, losing water and gas. (Uncontrolled catalytic recombination of the oxygen and hydrogen produced gas is a source of possible excess power in the right range.)

The MIT estimate of the excess power expected using the scaling methods of Utah applied to the MIT electrodes and current was 80 milliwatts. The point is clear: the expected nuclear products are excluded by the MIT paper at a level down from the Utah claims by several orders of magnitude. But MIT could limit the excess calorimetric power only by less than one order of magnitude, a factor somewhere between two and five. That entails much closer scrutiny of the much less precise results of calorimetry.

The hope of the optimists—Mallove is not the only one—is that the assumptions, plausible as they are, that fusion has to proceed at least in part along known channels, is somehow wrong. Heat is the most general (and desired) product. (Helium appears to me almost equally robust; if it is not made, the process is hardly fusion. It is possible that helium is lost from the cells.)

IV. The Two Drafts Differ

The focus of Mallove's criticism is on a difference between the two partial MIT draft manuscripts. In the earlier one (Attachment 5) the excess power is corrected by a simple linear fit for long-term drift and plotted, with many points, both for heavy water and the light water control cell. I measured on the MS graphs the mean excess power over time for both cells, using for each the area of the region between the locus of zero power and the noisy data.



From Philip Morrison's PBS Television Series

The mean power excess: heavy water cell +15 milliwatts; light water cell + 4 milliwatts

Neither result is significant, for the claimed power error is 40 mW, and even a less stringent definition would put the error at 20 or 30 mW.

The 13 July draft fits a less simple, least square correction to eliminate the drift, and ends up with a value I did not measure, but one visibly close to zero for both cells. The claimed error is not reduced; such small variations in the mean are simply not significant, whether they turn out zero or not.

The first hint of a small positive excess power in the heavy water cell is a source of encouragement for those who expect power from fusion, and its disappearance between drafts is the burden of Mallove's concern. But it might well have been that the new cor-

Now Energy Times Archive

rection would have shown the other sign. After all, the light water cell also indicated an excess power at first, if smaller still. These are matters of chance at this level of power.

Objectively, one has to say that the published paper does not mislead; the change in power between drafts is rather smaller than the clearly stated and plausible overall error, at most a little outside of random error alone. The disappointment of a hopeful reader is understandable, but hard to defend in reason. The open possibility might support an effort to do experiments with more precise calorimetry; they have gone on apace since, though not at MIT or at PFC.

V. My Recommendations

I close with an explicit answer to your questions about options for action in your letter of 9 October 91.

1) I do not believe an inquiry needs to be conducted.
2) I do not believe any formal investigation is needed.

Both of these recommendations apply only to the scientific paper, and not to the media interactions.

3) On the other hand, I recommend that PFC should spend a person-day or two of work to compute the mean excess power for all four cases, the light and heavy water cells under the two protocols of drift correction. They should also describe the two approaches to correction in more detail than the by-name-only account in the letter by Dr. Parker to Dr. Mallove of 8 August 1991. (Notice that in Figure 6 of the MS submitted from MIT, but not in the published version, one fit to the declining heater power curve is drawn.) The work need not even invoke much ado about old records; it can probably be done from the curves already published or at hand and the algorithms used.

Making these few numbers publicly available, first of all to Dr. Mallove—they are probably not important enough to publish—would for me fairly and helpfully clear the record. Anyone is then free to make what use he can of data that are clearly below the level of significance, if possibly suggestive to the hopeful.

Sincerely, Philip Morrison

Exhibit W
President Charles Vest's Letter to Eugene Mallove
October 17, 1991

President Vest tries to settle the matter with the Morrison memo. Nice try, but no cigar.—EFM

Dr. Eugene F. Mallove, Bow, New Hampshire

Dear Dr. Mallove:

As you know from my letter of October 15, 1991, I have asked Institute Professor Emeritus Philip Morrison to review the materials you sent me, in light of your concerns about the scientific content of investigations by MIT scientists into possible cold fusion phenomena. Professor Morrison has now done so and has submitted to me a thoughtful memorandum stating the issues as he views them and presenting his recommendations regarding an appropriate response for the Institute. I am sure that you join me in appreciating Professor Morrison's careful work in this regard.

Enclosed is a copy of Professor Morrison's memorandum, dated October 14, 1991. Before any further actions are taken by the Institute, I would like to have your reactions to his recommendations. Professor Morrison has kindly agreed to make himself available to discuss the content of his memorandum with you, if you so desire.

I hope that this represents a constructive step toward resolving your concerns.

Sincerely yours, Charles M. Vest

Enclosure
cc: Philip Morrison, Mary P. Rowe

Exhibit X
Eugene Mallove's Letter to President Charles Vest
October 24, 1991

I opposed the conclusions of the Morrison memo in very strong terms, asking again for a full-scale investigation. —EFM

President Charles M. Vest
MIT Room 3-208

Dear Dr. Vest:

Thank you for sending me Professor Philip Morrison's memorandum of 14 October to you regarding my 18 August request for an inquiry into scientific misconduct at MIT.

First, let me say that I have the highest regard for Professor Morrison as a gifted scientist and educator, a man of impeccable character, and one of the finest human beings I know. I also count him as a friend and a person who made an honest attempt to be fair to all sides in his letter to you. I deeply appreciate the time and effort he made on your and my behalf. With that said, I regret to tell you that I am in substantial disagreement with Dr. Morrison's conclusions about the technical matters that he discussed. These disagreements I detail below.

At your request, Professor Morrison did not concern himself with the substantial issues of motivation and dealings with the press by Professors Parker and Ballinger. That was a significant part of my request for an inquiry and investigation. Not only is it absolutely clear to me that these individuals engaged in reprehensible conduct in the manner that I have described and documented, but their behavior significantly illuminates the handling and representation of the technical aspects of their experiments. The PFC cold fusion experiments of 1989 were by no means conducted in an intellectual vacuum. There was a clear rush to judgement by that group and a calculated attempt by at least two of its members to besmirch the scientific work of Drs. Fleischmann and Pons. I urge you to immediately turn over this aspect of my allegations of misconduct to a panel of individuals who will assess these concerns fairly and make recommendations.

Now let me return to Professor Morrison's memorandum to you. I do not agree with his conclusions that there is no need for an inquiry and no need for an investigation. I have read Professor Morrison's comments carefully. With due respect for his prodigious scientific talent and respected judgement, I come to very different conclusions, for reasons that will become clear.

First, let me begin by noting Professor Morrison's initially stated bias, a term that I hasten to characterize precisely in imputing such to Dr. Morrison. It is certainly not a bias that arises from any animus, but it is a bias nonetheless. He writes, "I still believe that there may be a germ of novelty in some electrochemical phenomenon that is caught up in this complex system; it is very unlikely, though logically possible, that new findings, if established, would turn out to have high economic importance. They would at most open some way to build a new battery, possibly a fuel cell." I suggest to you and to Dr. Morrison that this flies in the face of the work of hundreds of researchers around the world—many working right now and getting remarkable results—scientists who have done experiments that clearly reveal nuclear processes at work where none should be, by conventional reckoning.

If Professor Morrison relies mainly on what he has read in *Nature* about cold fusion or what the people at the Plasma Fusion Center have told him, then that may explain his missing a host of phenomena that many observers, who did not reject cold fusion in the spring of 1989, see as firmly established. There are two classes of phenomena that in my view have been experimentally confirmed in deuterated metal systems: (1) Calorimetrically measured excess energies that exceed megajoules per mole (tens of MJ/mole has been reliably found) and (2) Nuclear anomalies that may well be linked to the excess heat, albeit not in naive or one-to-one correspondence to nuclear products that have been measured (there may be others not yet measured). Among the byproducts that have been found are tritium, neutron emissions (both burst and continuous), helium-4, and charged particles with MeV energies. There is a steadily growing literature of such findings, both thermal and nuclear effects. This cannot be brushed away. It must be evaluated and studied carefully, without the preconceptions that have plagued this controversy. The phrase, "They would at most open some way to build a new battery . . .," I have heard before. It was made by Dr. Frank Close, whose assessment of cold fusion deliberately did not include the vast bulk of supporting data.

In evaluating the MIT experiment, Professor Morrison repeats the canard used by those who have tried to "wish away" many cases of experimentally determined excess heat: "Uncontrolled catalytic recombination of the oxygen and hydrogen produced gas is a source of possible excess power in the right range." A number of excellent experiments have thoroughly investigated this "explanation" and have rejected it.

New Energy Times Archive

Recombination is not a significant factor in open cell work. (In closed-cell, deliberate recombination work, it is no factor at all, and there are excellent positive experiments of that kind too.) On the other hand and to his great credit, Professor Morrison correctly notes, "It is possible that helium is lost from the cells." Indeed, this is precisely the result that researchers at the Naval Weapons Center have recently reported in connection with their calorimetric work.

There is a significant omission in Professor Morrison's critique. Nowhere do I find an assessment of the validity of subtracting a "somewhat arbitrary linear function" from the raw heater power data to get net excess power. Dr. Morrison assumes *a priori* that the group employed a correct algorithm. But as my 18 August letter suggests, that methodology is completely incorrect.

Professor Morrison accepts what I believe to be a gratuitous statement in Professor Parker's hastily prepared letter to me of August 8, 1991—the one I received on the eve of the WBUR radio broadcast—in which he tries to explain the apparent curve shift between July 10 and July 13. Parker's statement is: "In one, the drift was fitted with a somewhat arbitrary linear function. . . in the other, the drift was fitted with a different linear function, this time a least squares fit, and the data appearing in the final version of the paper were produced." I do not think it is possible to accept this statement on its face. The light water curve has not been moved down by any new form of mathematical adjustment, but the heavy water curve most certainly has been moved. In fact, I believe that this statement about two methods of fitting the drift to be a complete fabrication on the part of Parker, much as his statements about what he did or did not say to Nick Tate are completely false.

Professor Morrison is certainly correct that the July 13 draft results in an excess heat that is "visibly close to zero for both cells." I do not contest that, for that is precisely what this curve shift was apparently intended to bring about. So, I must strenuously disagree with Professor Morrison's statement, "Objectively, one has to say that the published paper does not mislead." I believe to the contrary. The paper misleads most egregiously. Professor Morrison's polite recommendation #3 directly governs this matter: "They should also describe the two approaches to correction in more detail than the by-name-only account in the letter by Dr. Parker to Dr. Mallove of 8 August 1991." In my view, there is very strong evidence that the alleged "two approaches" is really only one method of fitting the data and performing the forementioned inappropriate subtraction of the resulting fitted line. The second "approach" is merely data-averaging into one-hour samples and subsequent shifting of the curve down to give the impression of a null result. More evidence for my contention: If there really had been two methods of analysis applied to the data that would instantly explain the curve shift, why would not Professor Parker *et al.* have immediately shown me the two computer source codes or the notebook-written algorithms. This would have immediately cleared up at least the matter of the origin of the shift. Such was not done, I am quite certain, because two distinct approaches to the data processing did not exist.

I do not believe that Professor Parker *et al.* can be trusted at this time to give forth a complete account of how that curve adjustment occurred. If asked to do so, they might contrive some kind of algorithm with a convenient free parameter or parameters that will show how they processed the data to get the two sets of results.

In conclusion, and in regretted disagreement with the recommendations of Professor Morrison, I must again ask you to convene an appropriate panel to thoroughly explore scientific misconduct on the part of the MIT research group. When this panel renders its opinion, I am confident that it will reach the very conclusion about this experiment that Professor Parker publicly announced on June 7, 1991, and later implicitly rejected, namely that it not "worth anything." This is what he said and he will be held to his word that he believes the curves can be redrawn "anyway that you want":

"I'll tell you what my opinion is of that work, because I was part of it. I don't think it's worth very much. Alright? And that's why it's just published in a tech report. I don't think it's worth very much. I think to do calorimetry is one of the hardest things I ever tried to do. I'd rather stick to plasma physics. . . When you have an open system is where you can make big errors, where you don't know the overpotential, the electrode potential, and so on. These things are unknown. I mean it's really tough and that's why I don't put any stock at all—you can redraw those curves anyway that you want. I don't think that data is worth anything."

Rather than allow the PFC team members further opportunities to fiddle with their data, I recommend that all notebooks, computer files, and printed records relating to their experiment be immediately impounded and turned over to an investigative team for thorough analysis. I requested this inquiry in late August. It is now late October. Your letter to me in early September said that my material would be "appropriately reviewed during the coming several days." Time enough has gone by in deliberations over what to do. It was a good, but not a sufficient course to ask Professor Morrison's opinion, which he rendered the day after he received the material from you. However, it is now time to appoint a panel that will probe to the core of what I steadfastly believe to be evident scientific misconduct.

Both you and others, especially Professor Morrison, should be under no illusion that my hope for cold fusion lies in extracting some evidence of excess heat from the PFC data. You should know that positive conclusions about cold fusion are based on a preponderance of evidence from elsewhere. After the results that will likely be discussed at the third annual conference on cold fusion in Nagoya, Japan, in the fall of 1992, no one will need the PFC's ancient and discredited data. It will be past history, and, indeed, a very sorry history for MIT.

The case of the PFC data concerns scientific ethics: Is it permissible to massage data to one's taste—to artificially present a strong negative impression rather than an ambiguous and possibly positive one? I think not. I do not deny the PFC group the right to discuss the sensitivity of their experimental measurement and then to suggest that because of that sensitivity their result should be read as null. But it was clearly inappropriate to arbitrarily shift processed data to contrive a "desired appearance" and not let the viewers of their report form their own judgements. And do not forget my contention that the very basis for arriving at the original processed data (before the shift) is, in any event, not correct. At the very least, the paper requires substantial revision on that account.

Until I am persuaded by strong technical arguments that deliberate curve-shifting has not occurred, and that an appropriate processing of the data has been applied, I will continue to believe in the need for a thorough investigation. I appreciate Professor Morrison's offer to discuss the content of his memorandum with me, however at this time I do not feel a need for that kind of discussion. The discussion I and several of my colleagues would respectfully like to have with him would be to discuss the overwhelming ancillary evidence for cold fusion, not the PFC paper. I understand his contentions about the PFC work very well and do not need clarification. I look forward to your reply about the next steps that you will take.

Sincerely, Eugene F. Mallove

Exhibit Y
Eugene Mallove's Letter to President Charles Vest
December 31, 1991

Over two months have gone by and still no action has been taken on my request for a full investigation.—EFM

President Charles M. Vest, MIT Room 3-208

Dear Dr. Vest:

My last letter to you regarding the progress of the MIT misconduct investigation was on 24 October. With the exception of a subsequent telephone message from Ms. Laura Mersky stating that you considered the investigation to be a matter of high priority, which you would attend to upon returning from a foreign trip, I have received no communication from you. Would you kindly let me know what actions have been taken?

I can assure you that in 1992 there will be many developments regarding cold fusion, both scientific and governmental. It will accordingly be imperative to resolve the MIT misconduct matter as soon as possible. I look forward to your early reply.

Sincerely, Eugene F. Mallove

Exhibit Z
President Charles Vest's Letter to Eugene Mallove
January 6, 1992

President Vest's penultimate brush-off letter, which suggests no need to do anything further. He checked with his legal counsel

New Energy Times Archive

regarding the matter of unethical press deception, which he calls "bias in dealing with the media."—EFM

CHARLES M. VEST, PRESIDENT, Room 3-208

Dr. Eugene F. Mallove, Bow, New

Dear Dr. Mallove:

In your letter of October 24, 1991 commenting upon Professor Philip Morrison's report on his inquiry into your concern about possible scientific misconduct in the paper by Albagli *et al.*, you acknowledge Professor Morrison as:

> a gifted scientist and educator, a man of impeccable character and one of the finest human beings I know. I also count him as a friend and a person who made an honest attempt to be fair to all sides in his letter to you. I deeply appreciate the time and effort he made on your and my behalf. With that said, I regret to tell you that I am in substantial disagreement with Dr. Morrison's conclusions about the technical matters that he discussed.

I agree with all you have to say about Professor Morrison and am left with the question of whether I should ignore his conclusions and order the additional investigation which you requested in your October 24 letter. Since receiving your letter, I have sought an additional independent review by Professor J. David Litster, our interim Associate Provost and Vice President for Research, and he has confirmed Professor Morrison's conclusion that there is no basis for further investigation of the charges of scientific misconduct. I have also considered, and sought advice from legal counsel, on whether it was necessary or appropriate for me to investigate your charges of bias in dealing with the media, and I have concluded that is neither necessary nor appropriate for me to do so.

You will recall that Professor Morrison did recommend "that PFC should spend a personal day or two of work to compute the mean excess power for all four cases, the light and heavy water cells under the two protocols of drift correction." However, your letter of October 24, 1991 rejects this suggestion, so I am not requesting that it be done.

In closing, I do wish to express my regret for the length of time that it has taken to respond to your letter of October 24. Because the Provost had participated in the Albagli paper, I thought it best if I dealt with your concerns personally, and between a long trip in November and the holidays, this is the first opportunity I have had to respond.

Sincerely yours, Charles M. Vest

CMV:cbb

Exhibit Z-1
Eugene Mallove's Letter to President Charles Vest
February 9, 1992

New evidence in the form of Dr. Mitchell R. Swartz's analysis of the MIT PFC data manipulation had come to my attention. My request for a thorough investigation became more emphatic.—EFM

President Charles M. Vest, MIT Room 3-208

Dear Dr. Vest:

I had intended to reply promptly to your letter of 6 January 1992, but new information was unexpectedly brought to my attention, which delayed my response. I am referring to the substance of the draft report, "Semiquantitative Analysis and Examination of MIT PFC Phase-II Cold Fusion Data," which MIT graduate Dr. Mitchell R. Swartz submitted to you on 28 January after a lengthy period of very careful consideration. He has discussed his analysis with me and given me a copy of the report that he submitted to you and to Professor Ronald R. Parker. Let me assure you that Dr. Swartz initiated his investigation independently. I was surprised—shocked would be a better word—to see what he found. I was given a copy of his report to verify the accuracy of Dr. Swartz's use of quotations from correspondence that I had allowed him to see. Later, I requested and received a copy of his draft report.

In your one-page letter of 6 January, you mention consultations with your colleagues, Professor J. David Litster and legal counsel. Based on these consultations and the previous memorandum submitted to you by Professor Philip Morrison, you presumed that you had dealt appropriately with my request for a misconduct investigation. I am sure that it will not surprise you to hear now that I emphatically disagree. I am extremely disappointed that you chose to sweep this serious matter under the rug with what I consider to be a totally inadequate response. I trust, however, that what Dr. Swartz's analysis has revealed about the MIT PFC "Phase-II" data will have dismayed you and your colleagues and will lead you to quickly reverse your dismissal of my charges. It seems that Dr. Swartz has performed a major part of the investigation that I believe should have been your business to require, but which you did not.

To summarize what I understand Dr. Swartz has discovered about the PFC data from his computer processing of the electro-optically scanned published and unpublished PFC results:

(1) Extra data points have been arbitrarily and inexplicably added to the published heavy water curve. These may amount to between 10 and 20 percent of the data points.

(2) The addition of two more "data points" at the portion of the curve, which in the July 10 pre-publication data exists as a time-calibration mark. These points were clearly arbitrarily placed in their vertical positions. They should not have been published as data points if they were mere time-calibration points. (This is not a small matter, see comment below.)

(3) Conclusive evidence that the light water data and the heavy water data were "processed" differently. The light water data that was finalized on July 13, 1989 is a direct and appropriate hourly average of the July 10th pre-publication data. By contrast, the finalized heavy water data of July 13 cannot be obtained from the July 10 prepublication data in a similar manner. Furthermore, Dr. Swartz has shown that no linear transformation exists that successfully maps the July 10 heavy water data into the final published data. This means that whatever technique was used to create the final published data set was highly contrived, that is, was manipulated to give the curve its final appearance. This is in contrast to the impression that Professor Parker has given all along, that the two data sets were treated either identically or in some kind of equivalent manner.

Dr. Swartz uses "polite language" to characterize these findings. He writes, "There appear to be the possibility that some of the data points in the published heavy water curves are most likely artifact, rather than a result of the original experimental data." He states, ". .there does appear to have been an asymmetric algorithm used when the July manuscripts are examined. The light water curve was published essentially intact, whereas the heavy water does appear shifted without any clear explanation for the difference." He writes of the heavy water data, ". . .the possibility of additional superimposed components cannot be excluded. . ." and ". . .much would be clarified by the marking of incidental, questionable, or less clearly derived data points."

I will not be so polite. To be blunt: Dr. Swartz's findings mean that it is absolutely certain that the heavy water experiment data have been manipulated, adulterated, and presented in a way that is completely misleading. I no longer characterize as scientific misconduct what I formerly believed to be an unwarranted shifting down of a data curve, which was a serious enough charge itself. I now consider that an individual or individuals responsible for the preparation of that data are guilty of scientific fraud. I am using the term fraud, which connotes in my mind a deliberate attempt to mislead, to correspond with the definition of "misconduct" in "Section 50.102 Definitions of Sub-Part A, Section 493 of the Public Health Service Act." Furthermore, I would consider any significant delay in dealing appropriately and severely with these findings to be an attempt to cover up scientific fraud.

Some further conclusions that one can reasonably infer from Dr. Swartz's findings: The artifactual time-calibration mark is alluded to in the PFC *Journal of Fusion Energy* paper in Figure 6— in the context of the description of the declining heater power curve. There is no such description in the previous figure in which the data is presented, Figure 5, nor could there have been, since it would obviously have been ludicrous to insert points that look like data and then say, "by the way, they are not really data, they are calibration points." I suggest that these points seem to enhance the impression of a wider y-axis data spread—a useful impression to give if one is trying to show that any possible excess power is within error bounds. It is inconceivable to me that these points were "inadvertently" put in. The only way that could have happened would have been if the time-calibration "data" were included erroneously in the purported algorithm that processed this data (which would then invalidate the entire purported "least squares" fit that Professor Parker has mentioned in one of his letters). These are dramatic outlying points that I believe may have been intentionally introduced.

New Energy Times Archive

I hope that in view of the seriousness of Dr. Swartz's findings, my assertions about them, and in the context of other major faults that I and others have alleged about the MIT PFC publication, you will now act immediately. You could put in place a formal investigation of the entire matter with a panel of experts from both inside and outside MIT, which should include people both favorably disposed to cold fusion, and those either not so disposed or neutral. But let me suggest that based on the facts that now appear so crystal clear, you should ask Professors Parker and Wrighton, who were the leaders of the research in question, to formally withdraw the entire "Phase-II" part of the Journal of Fusion Energy published paper, and the corresponding part of the PFC/JA-89-34 report, and all conclusions that derive therefrom.

I note that in the January 1992 issue of *Physics Today*, there is a news item (see attached) about the Council of the American Physical Society voting last November to adopt a set of guidelines "outlining professional conduct by physicists." The text of the guidelines is printed and contains several statements that I think are very pertinent to the PFC matter. One statement is: "Following publication the data should be retained for a reasonable period in order to be available promptly and completely to responsible scientists. Exceptions may be appropriate in certain circumstances in order to preserve privacy, to assure patent protection, or for similar reasons." The response of Professor Parker throughout this affair egregiously violates this guideline. For example, he has deliberately determined not to reveal the method of generating the heavy water curve of the "Phase-II" results.

In my 24 October 1991 letter to you I specifically suggested that obtaining the algorithms that processed the data would be important. So did Professor Morrison in his 14 October 1991 memo to you: "They should also describe the two approaches to correction in more detail than the by-name-only account in the letter by Dr. Parker to Dr. Mallove of 8 August 1991." In your 6 January letter to me you did not mention this key suggestion by Dr. Morrison, which I most certainly did not reject. Nor, as you stated in that letter, did I reject Professor Morrison's suggestion "that [the] PFC should spend a personal day or two of work to compute the mean excess power for all four cases..." I must say, you have inadvertently twisted the interpretation of one brief phrase that I wrote, "..in regretted disagreement with the recommendations of Professor Morrison..", which was made in the context of a request for a much more complete investigation, to imply that I did not want to see those alleged PFC algorithms or have the means computed.

Another guideline that the APS Council put forth also has direct application to the matter at hand: "Fabrication of data or selective reporting of data with the intent to mislead or deceive is an egregious departure from the expected norms of scientific conduct, as is the theft of data or research results from others."

In the issue of *Tech Talk* dated 5 February 1992 is published the MIT procedures for dealing with "Academic Fraud in Research and Scholarship." The statement is made: "In addition, the Provost has the authority to mitigate the effects of the fraud by withdrawing MIT's name and sponsorship from pending abstracts and papers and by notifying persons known to have relied upon any work affected by fraud." I expect that this will be carried out for the matter at hand once the final determination has been made.

I note further in the MIT-published statement: "An inquiry must be initiated immediately after an allegation has been made and must be completed within 60 calendar days of its initiation unless circumstances clearly warrant a longer period. If the inquiry takes longer than 60 days, the record of the inquiry shall include documentation of the reasons for exceeding the 60 day period." My initial request for an inquiry and possible investigation was submitted on 18 August 1991. There have been a number of written exchanges between us, including my 31 December 1991 letter in which I asked about the status of your inquiry, following my earlier letter of 24 October. Your letter of 8 January 1992 seemed to terminate the matter as far as you were concerned. Thus, more than 140 days have gone by in which only one MIT faculty member, Professor Morrison, spent part of October 13 and October 14 reviewing (incompletely) my requests. After October 24, but perhaps as late as the first week of January 1992, Professor Litster, the Associate Provost to Provost Mark Wrighton (one of the authors of the questioned research) "independently" reviewed the matter and "confirmed Professor Morrison's conclusion that there is no basis for further investigation of the charges of scientific misconduct."

In your 8 January 1992 letter you wrote, "I have considered, and sought advice from legal counsel, on whether it was necessary or appropriate for me to investigate your charges of bias in dealing with the media, and I have concluded that it is neither necessary nor appropriate for me to do so." Please go back and read my 18 August request for an investigation, because you have again inadvertently twisted my position. I was not and am not concerned about "bias in dealing with the media." Bias in dealing with the news media (such as selecting which newspaper one wants to favor with an interview) is precisely the objective and accepted (if not altogether proper!) role of a news office; I have no objection to such "bias." What I was talking about in my request was the investigation of giving false information in matters that are not small—giving false information to me to prepare an erroneous new release, giving false information to others in the News Office, and giving false information to the greater world about what was said to a reporter. In other words, a calculated attempt to discredit that reporter and newspaper by giving false information. Moreover, a denial of giving this false information persists. Do you not think it is time to do something about that? Is this level of expected personal integrity no longer important at MIT?

In summary, I consider that the inordinately extended inquiry up to this point has not met MIT's own proclaimed standards. I hope that in light of the newly developed information, this time you will understand my points quite clearly. This is a very grave matter which has in my view, I regret to say, already damaged MIT. You cannot sweep research fraud under the rug -- even if it is in the context of a claimed phenomenon with which you, Provost Wrighton, and several others at MIT have little patience. If you do not do the necessary investigative job yourself, I am convinced that others inevitably will.

I am also sending a copy of this letter to my former associate, Kenneth Campbell of the MIT News Office, because I know he is familiar with the ramifications of this type of situation, having witnessed, as did I, another well-known controversy that did not help the image of MIT.

Sincerely, Eugene F. Mallove

cc: Kenneth Campbell

Exhibit Z-2
Eugene Mallove's Letter to President Charles Vest
February 21, 1992

President Charles M. Vest, MIT Room 3-208

Dear Dr. Vest:

On February 10 you received a letter from me that discussed a serious issue connected with research at MIT. Have you initiated any actions on this matter? When might I expect a reply from your office?

Sincerely, Eugene F. Mallove

cc: Kenneth Campbell [MIT News Office]

Exhibit Z-3
Dr. Stanley Luckhardt's Letter to Professor Morrison
March 10, 1992

While President Vest continues to stonewall, a congenial memo is passed between Dr. Luckhardt (who still has control of the contested data) and Prof. Morrison—some "investigation"! Of course, this concerns the "discredited" cold fusion, so it is not important for MIT to observe the usual standards.—EFM

Plasma Fusion Center
Massachusetts Institute of Technology

Dr. Luckhardt
MIT News Photo

MEMO
TO: Prof. Phil Morrison
FROM: Dr. Stan Luckhardt
MIT Plasma Fusion Center, NW16-266
DATE: 3/10/92
SUBJECT: ANALYSIS OF CALORIMETRY DATA IN THE PAPER: D. Albagii *et al.*, *Journal of Fusion Energy*, 9, 133, (1990).

In this memo, I will go through the analysis of the calorimeter data from our 1989 experiment, and show how we arrived at the reduced data presented in our paper. As explained in our paper, the calorimeter used a feedback controlled heating element to maintain a constant temperature

New Energy Times Archive

in the electrolytic cells. Any production of "excess heat" would show up as a reduction in the heater power level. The heater power level could be accurately measured by monitoring the current and voltage applied to the resistive heating element. The signal of interest is then the heater power ($P_h = I_h \times V_h$). The level of excess heat claimed by Fleischmann & Pons for our conditions is ~79 mW, this excess was claimed to appear after an initial "loading period" of some hours or days. Thus, to reproduce the claimed effect, we would expect the heater power to undergo a change of the claimed magnitude after some days of "loading."

In our experiments, and those of others using the open cell type calorimeters, the heater power undergoes a steady drift caused by the loss of solvent from the cell. This loss is caused mainly by electrolytic decomposition and evaporation. As solvent is lost, the level of solvent in the cell drops, this causes the thermal conduction path from the solvent to the top of the cell to increase, thus increasing the thermal "resistance" of the cell and reducing the rate of heat flow out of the cell. To maintain constant cell temperature, the heater power must also decrease slowly. This base line drift trend can be seen in the raw heater power plots attached.

To analyze our heater power data, we first subtract the baseline drift, then any onset of anomalous heating would appear as an excursion from zero. In particular, in the attached Figs. 2&3 I show the raw heater power data P_H for the D_20 cell and the linear regression fit y_H to the raw data. In Fig. 4 the difference

$P_X = Y_H - P_H$ is shown. To remove the high frequency fluctuations, the data is time averaged, Fig. 5. We believe these rapid fluctuations are caused by the trapping and escape of gas bubbles from under the Teflon supports for the cell electrodes, (see drawing of the cell in our paper) and by condensation in the cell which causes water droplets to occasionally fall back into the cell. These effects cause fluctuations in the level of liquid and as explained above result in heater power fluctuations.

The time averaged data has one main feature, a slow variation having a 24 hour period. We believe this is caused by daily room temperature variations and/or some sunlight hitting the cells. Both the D_2O and H_2O cells exhibit this feature. Aside from this 24 hour period variation, the data is quite close to zero with some residual fluctuations of order 10-20mW. There does not appear to be any evidence of the claimed anomalous heating event of magnitude 79mW in Fig. 5. This conclusion was stated in our paper.

Sampling rate: As noted in the caption of Fig. 6 in our paper, the data sampling rate for the D_2O cell power was reduced at t=30hours. This was done to save disk space on the data acquisition computer. The regression analysis described above tends to weight the initial data more heavily because of its higher sampling rate. In the attached pages, I show a further analysis in which a uniform sampling rate is used throughout. In the attached Figs. 6-9 the data analysis is carried out again using the uniform sampling rate data and the final time averaged signal <Px> is plotted in Fig. 9. This signal is almost indistinguishable from the results of the original analysis shown in Fig. 5. So our conclusion from this analysis is the same as before.

The analysis of the H_2O cell data is shown in Figs. 10-14. Note the in Fig. 14 there is a residual, 24 hour period variation in the heater power. As discussed above we believe this is an error signal caused by the daily variation of the room temperature and/or variation in sunlight hitting the cells.

I hope this brief summary is of use to you, please feel free to contact me at 3-8606 if you need further information.

Exhibit Z-4
Dr. Charles McCutchen's Letter to MIT President Dr. Charles Vest
March 19, 1992

DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
PUBLIC HEALTH SERVICE,
NATIONAL INSTITUTES OF HEALTH
BETHESDA, MARYLAND 20892 Bldg. 8, Room 403

Dr. Charles McCutchen
MIT Photo

I had asked NIH physicist Dr. Charles McCutchen to give me his opinion of the MIT PFC experiment and my interchanges with MIT. Dr. McCutchen, who with MIT Professor Robert Mann had had his own battles with unethical establishments, obliged and wrote to President Vest supporting my position.—EFM

Charles M. Vest, Massachusetts Institute of Technology

Dear Dr. Vest,

Eugene Mallove has sent me material about the MIT cold fusion experiment. Mallove contends that calorimeter data were manipulated to suppress experimental evidence that excess heat was generated when heavy water was electrolyzed. I can see why he is disturbed.

The experiment was not a gem. Of the 1.8 to 2.1 watts that went into each electrolysis cell, 1.25 to 1.55 watts were supplied by the heater. Over two thirds of the power entering the cell did not go into electrolysis. Any signal from excess power would have been diluted by more than a factor three.

Worse was the variability of the total power input. Over an interval of 80 hours, the heater power declined progressively by almost 20%. The experimenters ascribe this to the steady fall in level of the solution ln the cell. This reduced the working area of the heat-loss path through the insulation surrounding the cell, and lowered the power needed to keep the cell at constant temperature.

Power generation by cold fusion, if lt happened, would likewise be revealed by a reduction in heater power. The potential for confusion is obvious.

In his letter of August 8, 1991 to Eugene Mallove, Ronald Parker wrote, "The implicit assumption was that we were looking for a fast turnon of the anomalous heat production and so it was legitimate to subtract out a slow baseline drift caused by depletion of the electrolyte." This would have been a reasonable position, had the experimenters stated it in the paper. They did not do so. The ordinates of their figures are labeled P_x, elsewhere defined as unknown (*i.e.* excess) power, zero is marked on the ordinates, and nowhere is it stated that the height and slope of the curves mean nothing.

Had they wanted to be able to detect constant or slowly varying excess power the experimenters should have improved their calorimeters to get rid of the baseline slope. Their calorimeter depends on the constancy of the thermal conductance of its insulation, and steps should have been taken to make it constant. They could have put a thermally conductive sleeve between the cell and its insulation so as to keep the area of the heat path constant. They could have kept the solution at a constant level in the cell. Better, they could have done both.

They did neither. Instead, they say they subtracted a linear ramp from the data for apparent excess power in each cell. How they picked the height and slope of the ramp is not stated. Because I took their graphs of excess power to mean what they said, I assumed that they thought their procedure would not have concealed constant or slowly varying excess power, had such occurred.

Like me, Philip Morrison took the paper's results at face value. He calculated for himself the mean excess power shown in the heavy and light water data in the draft of July 10, 1989. A little average excess power came from the light water cell and more from the heavy water cell, which suggests that the height of the curves was not intended to be meaningless. Had the procedure simply subtracted the best-fit ramp from each curve, both of these averages would have been close to zero.

The published paper shows negligible average excess power from either cell. The change between draft and published version is what would have happened had the ramps been adjusted to yield the result that hot fusion experimenters preferred.

I understand that the experimenters have been unwilling to explain their procedure when asked, and have refused to give others their data.

New Energy Times Archive

For its own good, and to restore some civility to a contentious field, MIT should look into (1) how its scientists came to perform and publish such a poor experiment, (2) why they either misdescribed their results, making them seem more meaningful than they were or used a subtle correcting procedure without describing exactly what it was, (3) how it came about that data from calorimeters with a claimed sensitivity of 40 mw converged, between drafts, after completion of the experiments, to within perhaps 5 mw of the result that hot fusion people would prefer to see. It might have been chance, but it might not.

—Dr. Charles McCutchen

Another piece of apparent *a posteriori* adjustment in the paper concerns the calibrating procedure. The draft said that dry nitrogen was bubbled through the electrolyte to stir it. Nitrogen from from a gas cylinder or from evaporating liquid is dry. But dry nitrogen would cool the cell by evaporation. The nitrogen should have been bubbled through water at cell temperature on its way to the cell. In the published paper, "dry" was missing.

For its own good, and to restore some civility to a contentious field, MIT should look into (1) how its scientists came to perform and publish such a poor experiment, (2) why they either misdescrlbed their results, making them seem more meaningful than they were or used a subtle correcting procedure without describing exactly what it was, (3) how lt came about that data from calorimeters with a claimed sensitivity of 40 mw converged, between drafts, after completion of the experiments, to within perhaps 5 mw of the result that hot fusion people would prefer to see. It might have been chance, but it might not.

I think all parties would agree that if the experimenters thought their method of baseline correction would not conceal constant or slowly varying excess power they should have explained it in detail. If, on the other hand, both the height and the slope of their records were meaningless, they should have said so. I believe this information, whichever it is, should now be published.

Sincerely yours, Charles W. McCutchen

cc: Dr. Eugene F. Mallove, Professor Philip Morrison

Hand-written note attached in copy to Eugene Mallove:

Dear Gene,

Here it is. I hope MIT does something other than stonewall. I think my request is reasonable. If the height and slope of the curves mean nothing, the experimenters should say so in a corrigendum. If the slope subtraction scheme somehow left meaningful slope and height, they should explain why this is so in a corrigendum.

You have my permission to copy and distribute my letter if you think it would help to get the matter straightened out.

Sincerely yours, Charles McCutchen

Exhibit Z-5
Prof. Morrison's Report to President Charles Vest
March 20, 1992

Yet another letter from Prof. Morrison to President Vest, concerning the MIT PFC experiment controversy and the new analysis by Dr. Mitchell Swartz. Morrison's conclusion that "though the procedure was described in only a few lines, a technically-prepared reader who uses the entire paper can work out the missing details to a good degree," is patently not true. The MIT PFC paper on the Phase-II calorimetry is fraudulently deceptive.—EFM

Department of Physics,
MIT, Cambridge, MA 02139

From: Philip Morrison, Institute Professor (emeritus)
To: Charles Vest, President
Response to Your Letter of 10 March 1992

I. Question and Answer

Your letter put to me a specific, rather narrow question, whose background is a recent painstaking study of a particular research paper (and two draft manuscripts) from MIT. The critical study, largely directed at a few specific graphs and their captions, was carried out by Dr. Mitchell Swartz, Weston, MA; its final date is 27 Jan 1992. The research paper itself, by Albagli *et al.*, with 16 co-authors, came from the MIT Plasma Fusion Center, and was published in the *Journal of Fusion Energy*, vol. 9, no. 2, p. 133, 1990.

You wrote me: "The question I wish you to examine is: Is the data reduction method that was used. . . [to produce certain curves in the published paper referenced above] satisfactorily described? "

My reply is this: though the procedure was described in only a few lines, a technically-prepared reader who uses the entire paper can work out the missing details to a good degree.

That reader would certainly be reassured by having for comparison the data for the heater power of the light water comparison cell. Those data were not in the published paper, though they were made available by Dr. Luckhardt in a letter of August 13,1991, sent by Director Parker of the Plasma Fusion Center to Dr. Mallove (Mallove Attachment #12).

But I do not think I should stop abruptly. As a physicist, I want to outline the logic of the procedure, address the results, and even add a little new matter. Dr. Swartz's study seems to me to warrant a fuller explanation for your records (to augment my first response) and for possible transmission to others you may wish to inform.

II. Source Documents Used

The letter and manuscript from Dr. Swartz are the direct basis for my comments. But it was valuable as well to use the August 18,1991 letter of Dr. Eugene Mallove to you, with its many attachments, and my letter of last October (harmlessly misdated in Dr. Swartz's study). Both of these were available also to Dr. Swartz, and cited in his Appendix.

I was also supplied through your office with a new and fuller account of the data treatment procedures, an account prepared by Dr. Stan Luckhardt of the Plasma Fusion Center, who carried out the original calculation (Luckhardt MEMO, 3/10/92). Dr. Luckhardt and I have spoken by phone as well.

I return all those documents for your files. I have destapled and restapled some of the papers, and made a few tick marks.

III. My Standing

I refer to my earlier letter for a full statement of my own "limitations and qualifications" as an assessor. I still believe that there may be a germ of electrochemical novelty in this complex system, though perhaps independent of deuterium and palladium.

IV. The Substantial Issue: A Shifting Thermal Baseline

The research paper dealt with the comparative release of various energies during the electrolysis of light and heavy water using cells with Pd cathodes. All interest here centers on only one product of the process, thermal power release, although most of the research, and three-fourths of the paper, was devoted to a search for a variety of other products, on which limits were set at much greater sensitivity than for heat.

The center of attention is one calorimetric result: a light-water cell and a matched heavy-water cell are followed over 60 to 80 hours. The calorimetry is not absolute; both cells were open for the release of

New Energy Times Archive

gaseous products, possibly carrying a small liquid entrainment. The necessarily changing level of electrolyte meant a changing internal cell resistance, and a changing heat flow from the cell. That heat flow was monitored by a feedback system, which controlled the current to a thermal heater inside the cell, acting to keep a constant cell temperature within. The electrical inputs were monitored as well.

A noisy, fast-fluctuating heater current records what happened in the gassy, bubble-stirred, perhaps transiently bubble-blocked, system. The signal noise sets a statistical limit to the accuracy of any thermal power measurement at about plus or minus 40 milliwatts; this result is stated clearly, though not in detail quantitatively supported. The graphs do show the eye just the rough amount expected, the usual more or less two-sigma band of plausible uncertainty.

But the cell fluid level slowly changes, and with it the observed heater power. One plot is given, (Figure 6), for the total heater power in the heavy-water cell. Plainly the heater power declines over the run of more than a week by several times the width of its noise band. The noise is reduced both by digital filtering (the data were mainly sampled every two minutes for some 80 hours), and then by binning those data points. The sloping mean baseline observed is then adjusted to form a new horizontal axis, the mean zero line, for excess cell power, "by fitting the drift with a linear function and subtracting from the signal." The procedure cannot disclose any constant power difference between the two cells, since the initial value of the baseline is set at zero within the visible noise. But any chance in power yield between the two cells over time would appear. No significant change appears during the long run to break the linear fall.

A couple of very modest but eye-catching peaks do appear at 24 hour intervals: they may imply an ambient temperature minimum around midnight.

The binned data comprise some eighty numbers, each plotted as a dot. An exact dot count, as expected, does not work. The binning--"time averaged over 1-hour blocks"--is subject to the usual inclusive or exclusive decisions, especially because the digital sampling rate was changed at one time, as stated in the caption to Figure 6. Whether there are 45 or 43 dots in one forty-hour period is not material; these statistics cannot show such a nicety.

Another point seems important to flag. The heater power measured goes to supply most of the heat loss from the constant temperature cell. The more power supplied, the less power comes from internal cell processes. Now, the steady downward drift in the heater-power baseline for the heavy-water cell is slower than the similar drift seen for the light water cell, at only about 60% of the light-water rate. If no correction were made for that linear power decline, there would appear to be a higher "excess power" developed by the light water comparison cell, not by the one with heavy water. (I believe the Noninski paper (Mallove, Attachment 8) omits all evaporative effects.)

Greater fluid loss takes place in light water, presumably by evaporation, as expected because of its higher vapor pressure.- I have not been able to make a reasonably simple model to fit the presumed evaporative losses, but a crude estimate shows that evaporation of a water mass comparable to the electrolytic loss is not excluded either by cell power or by plausible gas-flow rate, estimated from saturation water vapor that may be carried both by gas bubbles and from the free cell surface. The data are not complete enough to allow a simple theory to include both the heat flow changes and any resistive effects of water loss and level change. The authors also did not offer any quantitative model for the empirically quite linear drift in power, though they outline the issues clearly. (The high-frequency heater power noise differs markedly between the two cells as well. There is a UROP study to be done here some day.)

V. Short-Time Confirmation

The week-long runs have attracted all the comment. But the published paper also presents full data for a short-time test that directly compared the measured heat production in light and heavy water cells over a time so short that the slow change in water level can be neglected. In Figure 3 the noise power is again about 40 milliwatts; the light- and heavy-water cell powers agree to well within that limit. The effect expected on scaling the Utah results would be double that, and should be visible if present.

This single test takes on a special interest because it was made at the end of the long run, after about 200 hours of electrolysis. If slow gas charging of the palladium electrode is a determining parameter, this was the likely optimum for the experiment reported. The hydrogen content of the Pd electrodes was measured after the experiment by degassing; the loading factor found was 75 to 80 %. If higher loading still is a necessary condition for excess heat, this early negative result could not in itself be final. That objection remains true for any negative test result until a necessary state of the electrode has been fully characterized!

VI. Recommendations

1. The full file I have seen (*i.e*, the papers of Mallove, Swartz, and the Luckhardt memo), including your queries to me and my own two responses, should be available to interested persons on request. True, that is a lot of paper; they could be given a listing and offered a choice.

2. Dr. Luckhardt should be encouraged to prepare an account of the drift correction based on his March 10, 1992 memo to me, perhaps adding a brief introduction, and Dr. Parker asked if he would issue it as a brief amplifying note from the Plasma Fusion Center. That can be sent by you to anyone who has written for more information, including of course the people who have already done so.

3. I hope that everyone will cool his comments: enough of acrimony. There are plenty of data from this powerful early experiment, though puzzles about the complex system remain even after two more years of widespread reports.

Exhibit Z-6
MIT President Dr. Charles Vest's Letter to Eugene Mallove April 1, 1992

President Vest's final brush-off of my request for a full investigation appropriately fell on April 1. Note how he conveniently puts Dr. Swartz's analysis off limits for discussion.—EFM

CHARLES M. VEST, PRESIDENT, Room 3-208

Dr. Eugene F. Mallove, Bow, New Hampshire

Dear Dr. Mallove:

I write in response to your letter of February 9, 1992. Earlier, by letter dated January 6,1992. I had responded to your letters of August 18 and October 24, 1991. I believe the issues raised by your earlier letters have been addressed, and I will not repeat that response here.

As for your comments and requests related to the letter and manuscript of Dr. Mitchell R. Swartz, it is not appropriate for me to comment to you on Dr. Swartz's work.

Sincerely yours, Charles M. Vest

CMV/mmd

Exhibit Z-7
Prof. Widnall's Letter to Dr. Charles McCutchen April 2, 1992

Now Charles Vest begins to act through subordinates, such as the new Associate Provost, Prof. Widnall, who would later become U.S. Secretary of the Air Force. She tells Dr. McCutchen, "the experimenters have been extremely forthcoming with Dr. Mallove," which clearly does not square with the facts as is evident by all previous exhibits.—EFM

Courtesy U.S. Air Force

OFFICE OF THE PROVOST
SHEILA E. WIDNALL, ASSOCIATE PROVOST
ABBY ROCKEFELLER MAUZE PROFESSOR
OF AERONAUTICS AND ASTRONAUTICS
ROOM3-234

Mr. Charles W. McCutchen
Department of Health, Education, and Welfare
Public Health Service, National Institutes of Health
Bethesda, Maryland

Dear Dr. McCutchen:

President Vest has passed your letter regarding the experiment reported in Albagli *et al.* on to me as falling within my responsibilities

New Energy Times Archive

as Associate Provost. I assume that you have written as an interested scientist and not as an official of NIH, which as far as I know, has no official interest in this topic nor any role in the sponsorship of the work.

As you are no doubt aware, the paper in question has been the subject of some scientific debate and media attention. And that is entirely proper since the nature and importance of scientific contributions are quite properly dealt with through open debate in the scientific literature, through peer review, in open scientific meetings and through the media. Disputes are part of the scientific process and scientific results are always provisional, based on the data and theories developed to date. The paper you questioned was a contribution to this debate but clearly not the last word.

I think MIT management should take a hand. In the end, this would waste less of its time than trying to stay out of the matter. It should ask the experimenters to publish a corrigendum saying that their original description obscured the fact that they were looking for an abrupt turn-on of power generation by the cell. I see no respectable reason for them not to comply. —*Dr. Charles McCutchen*

I believe that most of the issues raised in your letter are more appropriate for a letter to the editor of the journal in question or for a communication directly to the authors rather than a subject for action by MIT. MIT has separately considered the issues raised by Dr. Mallove. Contrary to the viewpoint expressed in your letter, the experimenters have been extremely forthcoming with Dr. Mallove. He has been given data from the calorimeter experiment and several opportunities to discuss the procedure used with the investigators.

In the near future, a memo will be prepared by the experimenters giving more details regarding the analysis of calorimetric data than were available in the manuscript.

I shall see that you receive a copy of this when it becomes available.

Sincerely, Sheila Widnall

Exhibit Z-8
Dr. Charles McCutchen's Letter to Prof. Sheila Widnall
July 26, 1992

Dr. McCutchen has not been impressed with Prof. Widnall's letter. He makes a simple request for an ethical clarification by the MIT PFC of its results. This suggestion was never carried out by MIT, thus maintaining the fiction, to this date, that the MIT PFC results of 1989 were definite and null for excess heat.—EFM

Sheila E. Widnall, Associate Provost
Massachusetts Institute of Technology, Room 3-234

Dear Dr. Widnall

Thank you for your letters and for talking with me on the telephone. I took your advice and telephoned Stanley Luckhardt.

As you probably know, because of the change in the thermal resistance between interior and exterior of its cell with time, the MIT cold fusion experiment could not have detected small, steady power production or small power production that varied linearly with time. However, Luckhardt says that the experiment specifically looked for a sudden onset of power production by the cell. Given this limited purpose, it was legitimate for the experimenters to subtract the the best-fit ramp from the data. So far, so good.

But the description of the experiment does not say that a sudden onset was all the experimenters were looking for. And it never says that ramp subtraction renders meaningless the height and slope of the resulting curve. Someone as sharp as Philip Morrison integrated the excess power signal numerically over the entire length of the experimental runs in the apparent expectation that the result meant something.

To the experimenters, a reduction in the integrated power between one draft and the next would mean only that their ramp subtraction had improved. To a reader who thought the absolute height of the curve was significant, this same reduction could look like data-cooking.

I therefore suggested to Luckhardt that science and civility would be served if the experimenters published a correction explaining that the height and slope of their final data meant nothing, and apologizing for not saying so in the original article. To my great surprise, he objected strongly to this proposal. For one thing, he said, in one part of their experiment, absolute signal height was significant. They had compared the excess power before and after fluid was electrolysed and then replaced. Somehow this was a reason for not explaining that, for most of the data, absolute height was not significant. More surprisingly, reference to the article shows that this absolute-height part of the experiment failed. The data were discrepant, though the experimenters thought that the discrepancy would be smaller if more care were taken to refill the cell to its original level.

I talked to Eugene Mallove, and found him unwilling to concede that the MIT experimenters' sin might be only misdescribing their experiment. He and Mitchell Swartz insist that comparison of the experimenters data before and after reduction shows that the reduction was not done properly, and that the effect was to suppress evidence of excess power that had a sudden onset. I think the experimenters had such a low opinion of both cold fusion and their own experiment that they would not have gone to the trouble of subtly cooking the results. Still, Luckhardt's adamant and puzzling refusal to clear up the confusion in their description make me wonder if they are keeping everything closed because there are things in their data reduction that will not stand examination.

So what should MIT do? Leaving the experimenters and the doubters to resolve the matter is creating a festering sore and a suspicion of coverup. The groups are not communicating effectively. Each accuses the other of non-cooperation.

I think MIT management should take a hand. In the end, this would waste less of its time than trying to stay out of the matter. It should ask the experimenters to publish a *corrigendum* saying that their original description obscured the fact that they were looking for an abrupt turn-on of power generation by the cell. I see no respectable reason for them not to comply. At the same time, they should be asked to give their original data, data reduction formulae and algorithms to the doubters, who should, in turn, be asked to give their objections, in writing, to the experimenters. Out of this head-knocking, truth should emerge.

These requests could not reasonably be refused. They are impolite, but both sides have broken social conventions (the doubters said the experimenters were crooks; the experimenters ran a party celebrating the death of cold fusion), so neither can expect the protections of politeness.

Sincerely yours, Charles W. McCutchen

Exhibit Z-9
Prof. Widnall's Letter to Dr. Charles McCutchen
August 3, 1992

Provost Windall's final stonewall letter to Dr. McCutchen.—EFM

Mr. Charles W. McCutchen
Princeton, New Jersey

Dear Dr. McCutchen:

I'm responding to your letter of July 26. I'm glad that you took the opportunity to speak with Dr. Luckhardt regarding his memo and the earlier paper. I recognize that this area remains controversial and the issue you raised is: Is there anything that MIT as an institution should do in response to the controversy?

MIT, along with all other universities that I know anything about, does not often intrude between its faculty members and their professional actions as scientists. We don't, for example, review the manuscripts of our faculty prior to publication, as do many corporations and government organizations. We are used to a high level of controversy, often between members of our own faculty. Disputes about scientific data, methods and results are common and play a positive role in advancing science. When MIT faculty take public positions as scientists or citizens, it is assumed that they are acting as individuals and not as official spokesman for the institution.

Criteria for institutional involvement in such matters derive from our contractual and institutional responsibilities. Alleged violations of institutional policies by members of our community will bring forth an institutional response. As you undoubtedly know, at our request Prof. Philip Morrison undertook a detailed examination of the issues raised by two

New Energy Times Archive

individuals concerning the manuscript in question and determined that there was no tangible basis for further institutional action.

I hope that the various groups on our campus who are involved in this research will continue to have collegial, scientific dialogues but I see no basis to direct any of the groups to take specific actions.

Sincerely,
Sheila Widnall
Associate Provost and
Abby Rockefeller Mauze Professor of
Aeronautics and Astronautics

Exhibit Z-10
Dr. Charles McCutchen's Letter to Eugene Mallove
August 18, 1992

Eugene F. Mallove, Bow, NH

Dear Dr. Mallove,

As you can see from the enclosures, I did not get far with Sheila Widnall. I was surprised she did not respond to my point that Philip Morrison, their own expert, had been misled by the paper. I had previously been surprised when Stanley Luckhardt irritatedly refused to consider publishing a full description of the way the experimenters interpreted their calorimetry experiment. I thought this would be a good way to remove some confusion and lower the anger level.

How about taking the advice Dr. Widnall offered in her letter of April 2, 1992, and submitting a letter to *Journal of Fusion Energy*. You might get your points out ln the open for the experimenters to answer. Perhaps the hot fusioneers will stop the *Journal* from publishing your letter. This would be objective evidence that they are brass-knuckle types, evidence you could take to Sheila Widnall to show what happens when one tries to have an "open debate in the scientific literature, through peer review," with MIT scientists.

Sincerely yours, Charles W. McCutchen

Exhibit Z-11
Dr. Charles McCutchen's Letter to Prof. Sheila Widnall
August 18, 1992

Dr. McCutchen's final word to Provost Widnall fell on deaf ears.—EFM

Sheila E. Widnall, Associate Provost
Massachusetts Institute of Technology, Room 3-234

Dear Dr. Widnall,

Thank you for your letter of Aug. 3, 1992. I do not envy you in having to deal with matters that take time away from the constructive business of the university. But consider, it is the enforcement of decency among scientists that makes collegiality possible. Without sanctions for bad behavior, science becomes a jungle. Like it or not, you knuckle-rappers are keepers of the flame.

MIT is using formal procedures to evade responsibility. You and I agree, I think, that bad scientific ethics are a university's business. So far, so good. But MIT thinks that getting Philip Morrison to give the matter a once-over-lightly discharges its responsibility. (Substitute '"Eisen" for "Morrison" and you have the beginning of the Baltimore affair.) I explained that Morrison was misled by the inaccurate description of the experiment, the very thing that I object to. That he was thus misled shows that a correction should be published. You did not respond. This is stonewalling.

Collegial mechanisms will not resolve the issue. They work when all parties play fair. The MIT hot fusion people are not playing fair. They published a misleading description of an experiment. The errors ln the description were important. The collegial rules require that they publish a clarification. They refuse to do so.

What can the cold fusion people do now? If they submit a complaining letter to the journal that carried the original paper, the hot fusion people will probably try to prevent it from being published, and likely succeed. There will be more anger on both sides, and a lot less collegiality—all because MIT management cannot bring itself to make the hot fusion people, its own employees, behave like gentlemen. (Remember that what they refuse to publish is their own, verbal description of the way the major part of the experiment worked.)

I am sorry that MIT continues to tough it out. Apparently the university feels it need not be fair to cold fusion people. Perhaps it is afraid to be fair. Luckhardt's negative response to my proposal that a correction be published suggests that hot fusion patriotism *requires* one to be unfair to cold fusion people. Why else (unless there is real data faking that they are trying to hide) will the hot fusion people not publish a correction that would blunt some of the anger, and enhance their own reputation for honesty?

Sincerely yours, Charles W. McCutchen

cc: Dr. Eugene F. Mallove

Press Release
MIT News Office
April 12, 1989

"MIT Professor Has a Theory to Explain 'Cold Fusion'"

MIT Provost John Deutch said, "MIT is a place where creative individuals are encouraged to address scientific subjects of the greatest significance. We are pleased to see Professor Hagelstein proposing an explanation for 'cold fusion' and we are encouraging investigators both here and at other research institutions to continue their work on this most surprising phenomenon, which may have enormous consequences."

[Later, Dr. Deutch became Director of the CIA in the Clinton Administration and had nothing further to say or do about cold fusion—as far as we know].

Ponder the unthinkable. Question the status quo. Live in the world as well as in your own nation. Dream of a better future, but contribute to the present. Share your talents. Commune with all people. Be steady friends and bold companions. Be honest in all that you do.

—MIT President Charles Vest's Commencement Address June, 1998.

Why MIT continued from page 65

cisely because in their minds MIT's "smart people" back in 1989 proved that there was nothing to cold fusion claims? Lots. We hear from them all the time. When they first learn about what is happening in the cold fusion field today they are shocked, and then amazed to hear of the farce at MIT in 1989 and beyond.

MIT's Plasma Fusion Center continues to receive tens of millions of dollars per year for its tokamak hot fusion program. This clearly makes no sense now that cold fusion has begun to demonstrate its commercial potential as both an energy source and in the low-energy remediation of radioactivity generated in the past from the commercial and defense industry fission nuclear enterprises. Who would wish to waste further billions of dollars on a technology—hot fusion—that has already come under serious question for its technological and economic viability as a twenty-first century energy source, *IF* there were a clear alternative?

The alternative is here. Come home to your traditions, MIT. Arise ye sons and daughters of MIT—help bring MIT back to excellence and intellectual integrity on a new science frontier.

New Energy Times Archive

Key Cold Fusion Publications of MIT Graduate, MIT Professor Peter L. Hagelstein

• "Coherent Fusion Theory," presented at the ASME Winter Meeting, San Francisco, Dec. 1989, paper TS-4.
• "Coherent Fusion Theory," *Journal of Fusion Energy*, Vol. 9, No. 4, 1990. pp. 451-463.
• "Status of Coherent Fusion," DoE Annual Report, January 1990.
• "Status of Coherent Fusion Theory," *Proceedings of The First Annual Conference on Cold Fusion*, March 28-31, 1990, Salt Lake City, pp. 99-118.
• "Coherent Fusion Mechanisms," AIP (American Institute of Physics) *Conference Proceedings #228, Anomalous Nuclear Effects in Deuterium/Solid Systems*, Provo, Utah, 1990, Editors: Steven E. Jones, Franco Scaramuzzi, and David Worledge, pp. 734-781.
• "Coherent and Semi-Coherent Neutron Transfer Reactions," Conference Proceedings, Vol. 33, *The Science of Cold Fusion*, Ed: T. Bressani, E. DelGiudice, and G. Preparata, SIF Bologna, 1991, pp. 205-209.
• "Coherent and Semi-Coherent Neutron Transfer Reactions,"*Proceedings of the Third International Conference on Cold Fusion (October 21-25, 1992), Frontiers of Cold Fusion*, Ed., Hideo Ikegami, Universal Academy Press, Inc., Tokyo, pp. 297-306.
• "Coherent and Semi-Coherent Neutron Transfer Reactions I: The Interaction Hamiltonian," *Fusion Technology*, Vol. 22, 1992, pp. 172-180.
• "Coherent and Semi-Coherent Neutron Transfer Reactions III: Phonon Generation, "*Fusion Technology*, Vol. 23, 1993, p. 353-361.
• "Coherent and Semi-Coherent Neutron Transfer Reactions II: Dipole Operators," submitted to*Fusion Technology*, 1993.
• "Coherent and Semi-Coherent Neutron Transfer Reactions IV: Two-Step Reactions and Virtual Neutrons " submitted to *Fusion Technology*, 1993.
• "Lattice-Induced Atomic and Nuclear Reactions," *Transactions of Fusion Technology, Proceedings of the Fourth International Conference on Cold Fusion* (Lahaina, Maui, Hawaii, December 6-9, 1993, Vol. 26, No. 4T, December 1994, pp. xi-xii.
• "In Memory of Julian Schwinger," *Transactions of Fusion Technology, Proceedings of the Fourth International Conference on Cold Fusion* (Lahaina, Maui, Hawaii, December 6-9, 1993, Vol. 26, No. 4T, December 1994, pp. xi-xii.
• "A Possible Mössbauer Effect in Neutron Capture," *Hyperfine Interactions*, Vol. 92, 1994, p. 1059-.
• "Update on Neutron Transfer Reactions," *Proceedings of the Fifth International Conference on Cold Fusion* (9-13 April 1995, Monte Carlo, Monaco), pp. 327-337.
• "Proposed Novel Optical Phonon Laser Pumped by Exothermic Desorption," *Bull. APS*, Vol. 40, 1995, p. 808.
• "Anomalous Energy Transfer Between Nuclei and the Lattice," *Progress in New Hydrogen Energy: Proceedings of the Sixth International Conference on Cold Fusion*, October 13-18, 1996, Japan, pp. 382-386.
• "Models for Anomalous Energy Transfer,"*Proceedings of the Seventh International Conference on Cold Fusion*, Vancouver, B.C., April 19-24, 1998, pp. 140-146.

Key Cold Fusion Publications of MIT Professor Keith H. Johnson

• "Hydrogen-Hydrogen/Deuterium-Deuterium Bonding in Palladium and the Superconducting/Electrochemical Properties of PdH/-PdD," K.H. Johnson and D.P. Clougherty, *Mod. Phys. Lett. B*, Vol. 3, 1989, p. 795-.
• "Jahn-Teller Symmetry Breaking and Hydrogen Energy in γ-PdD 'Cold Fusion' as Storage of the Latent Heat of Water," K.H. Johnson, *Transactions of Fusion Technology, Proceedings of the Fourth International Conference on Cold Fusion* (Lahaina, Maui, Hawaii, December 6-9, 1993, Vol. 26, No. 4T, December 1994, pp. 427-430.
• "Method of Maximizing Anharmonic Oscillations in Deuterated Alloys," U.S. Patent 5,411,654, Brian S. Ahern, Keith H. Johnson, and Harry R. Clark, Jr., Filed July 2, 1993, Date of Patent, May 2, 1995.
• "Water Clusters and Uses Therefore," K.H. Johnson, Bin Zhang, and Harry C. Clarke, US Patent 5,800,576, Filed, November 13, 1996, Date of Patent, September 1, 1998.

Cold Fusion Publications of MIT Graduate Dr. Mitchell R. Swartz

JET Energy Technology has contributed through our R&D, high standards, and quality control.

Publications on Research and Q/C

• Swartz, M. 1993. "Some Lessons from Optical Examination of the PFC Phase-II Calorimetric Curves," Vol. 2, *Proceedings: Fourth International Conference on Cold Fusion*, 19-1, *op. cit.*
• Swartz, M. 1994. "A Method To Improve Algorithms Used To Detect Steady State Excess Enthalpy," *Transactions of Fusion Technology*, 26, 156-159.
• Swartz, M. 1996. "Relative Impact of Thermal Stratification of the Air Surrounding a Calorimeter," *Journal of New Energy*, 2, 219-221 (1996)
• Swartz, M. 1996. "Improved Calculations Involving Energy Release Using a Buoyancy Transport Correction," *Journal of New Energy*, 1, 3, 219-221.
•Swartz, M. 1996. "Potential for Positional Variation in Flow Calorimetric Systems," *Journal of New Energy*, 1, 126-130.
•Swartz, M. 1996. "Definitions of Power Amplification Factor," *J. New Energy*, 2, 54-59.
• Swartz, M. 1997. "Consistency of the Biphasic Nature of Excess Enthalpy in Solid State Anomalous Phenomena with the Quasi-1-Dimensional Model of Isotope Loading into a Material," *Fusion Technology*, 31, 63-74.
• Swartz, M. 1997. "Noise Measurement in Cold Fusion Systems," *Journal of New Energy*, 2, 2, 58-61.
• Swartz, M. 1997. "Biphasic Behavior in Thermal Electrolytic Generators Using Nickel Cathodes," *ECEC 1997 Proceedings*, paper #97009
• Swartz, M. 1998. "Patterns of Failure in Cold Fusion Experiments," *Proceedings of the 33rd Intersociety Engineering Conference on Energy Conversion*, IECEC-98-1229, Colorado Springs, CO, August 2-6,1998.
• Swartz, M. 1998. "Optimal Operating Point Characteristics of Nickel Light Water Experiments," *Proceedings of ICCF-7*.
• Swartz, M. 1998. "Improved Electrolytic Reactor Performance Using p-Notch System Operation and Gold Anodes," *Transactions of the American Nuclear Society*, Nashville, TN 1998 Meeting, (ISSN:0003-018X publisher LaGrange, IL) 78, 84-85.

Publications on Quasi-1-dimensional Isotope Loading, and Optimal Operating Point Behavior

• Swartz, M. 1992. "Quasi-One-Dimensional Model of Electrochemical Loading of Isotopic Fuel into a Metal," *Fusion Technology*, 22, 2, 296-300.
• Swartz, M. 1994. "Isotopic Fuel Loading Coupled to Reactions at an Electrode," *Fusion Technology*, 96, 4T, 74-77
• Swartz. M. 1994. "Generalized Isotopic Fuel Loading Equations," *Cold Fusion Source Book*, International Symposium on Cold Fusion and Advanced Energy Systems, Ed. Hal Fox, Minsk, Belarus,
• Swartz. M. 1997. "Codeposition of Palladium and Deuterium," *Fusion Technology*, 32,126-130

Publications on Catastrophic Desorption and Nuclear Theory

• Swartz. M. 1994. "Catastrophic Active Medium Hypothesis of Cold Fusion," Vol. 4. *Proceedings: Fourth International Conference on Cold Fusion*, sponsored by EPRI and the Office of Naval Research.
• Swartz, M. 1996. "Possible Deuterium Production from Light Water Excess Enthalpy Experiments Using Nickel Cathodes," *Journal of New Energy*, 3, 68-80 (1996).
• Swartz, M. 1997. "Hydrogen Redistribution by Catastrophic Desorption in Select Transition Metals," *Journal of New Energy*, 1, 4, 26-33.
• Swartz, M. 1997. "Phusons in Nuclear Reactions in Solids," *Fusion Technology*, 31, 228-236 (March 1997).

Edson continued from page 93

enjoys the thought of no longer daily commuting 60 miles to Cambridge from his home in Bow, New Hampshire. He has a dozen book proposals making the rounds of publishing houses and if none of them works out, he could return, he says, to an early love—the entrepreneurial life. In the 1980s he produced astronomical materials such as sky maps for museums and consulted with aerospace manufacturers like Hughes on the potential of innovative space propulsion systems.

At the moment he has no special plans for cold fusion except to write a sequel to his book—assuming, of course, that there is also a sequel to cold fusion.

New Energy Times Archive

Electric Spacecraft Journal Network

Keeping you informed about independent research...

The ESJ Network assists individual research efforts, facilitates cooperation among experimenters and provides an open forum for the exchange of information and ideas.

The Network welcomes those engaged in experiments, analysis, conceptual thinking, and investigative research, who are willing to interact with others. Inquiries to: 828-683-0313 or 828-683-1280, Fax: 828-683-3511; **Subscriptions:** [quarterly] *Electric Spacecraft Journal*, 73 Sunlight Drive, Leicester, NC 28748 U.S. annual subscription rates: $24; Canada and Mexico $29; Overseas $39.

The **Planetary Association for Clean Energy**, Inc.

Since 1975 ...
...a collaborative network of advanced scientific thinking

advanced therapeutics
Brown's Gas
consciousness studies
electromagnetic field bioeffects
free energy devices
human rights of scientists
legacy of Nikola Tesla
nuclear and toxic waste decontamination
scalars & the fundamentals of physics

Ask for a sample copy of our Newsletter with a book, video, software & CD-ROM list.

The **Planetary Association for Clean Energy**, Inc.
100 Bronson Avenue, Suite 1001,
OTTAWA, Ontario K1R 6G8 Canada
(613) 236-6265; fax: **(613) 235-5876**

Preview 2000
40 Zero-Emission Vehicles for the New Millennium

Electrifying Times
Latest Electric Car News

One year subscription, $12.00
(541) 388-1908
63600 Deschutes Road
Bend, OR 97701

etimes@teleport.com

EXOTIC RESEARCH REPORT

21st Century
Energy Technology
Alternative Medicine
Latest Inventions

U.S. $30
Can/Mex. $35.
Foreign: $55.

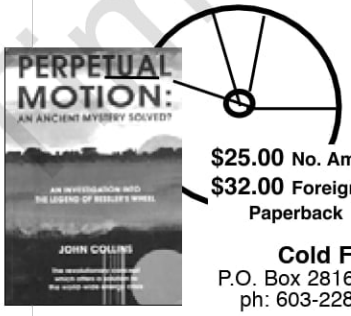
Exotic Research
P.O. Box 411
Stanfield, AZ 85272
1-800-417-6399

Cold Fusion & New Energy Lectures

by Dr. Eugene F. Mallove
Editor-in-Chief
***Infinite Energy* Magazine**
and author of *Fire From Ice,*
Generate lots of excess heat (and new-thinking!) in your scientific, engineering, business, civic, or religious organization.

Give a gift subscription of *Infinite Energy* to a school or library.

ORDER
PERPETUAL MOTION:
An Ancient Mystery Solved?



$25.00 No. Amer.
$32.00 Foreign
Paperback

"The book deserves to be on many a history of science shelf, not only for the mechanical puzzle it poses but also for the timeless human story it tells... "
Dr. Thomas Phipps, Jr.

Cold Fusion Technology
P.O. Box 2816, Concord, NH 03302-2816
ph: 603-228-4516; fax: 603-224-5975

Limited Edition CD! Antigravity Cold Fusion Ball Lightning

Runs on any computer with a browser

- Theory
- Stories, reports
- Pictures taken at LANL, NASA, Puthoff's Institute, Wright Patterson, and ENECO
- Movies of Stenger's ball lightning experiments, lobby at PowerGen, lectures, Cutlass Submarine, plasma ball in a microwave chamber, cryogenic zero point energy experiments/index

$19.95 No. America
$ 23.95 Foreign
Order from:
Cold Fusion Technology, Inc.
P.O. Box 2816,
Concord, NH 03302-2816
Phone: 603-228-4516;
Fax: 603-2284-5975
http://www-infinite-energy.com

New Energy Times Archive

CALENDAR

Past, Present and Future

CONFERENCE ON FREE ENERGY

U.S. Department of State, 2201 C Street, NW, Washington, DC 20520
Acheson Auditorium, **April 29 and 30, 1999, 9 a.m. - 6 p.m.**
Workshops, May 1, 1999 at hotel. Co-sponsored by Integrity Research Institute.
For further information and schedule of events:
call IRI at 800-295-7674, fax: 301-513-5728, e-mail iri@erols.com
Pre-registration required for admission.

Scheduled Plenary Speakers:

Les Adam, "Magnet Power and Non-Combustive Helicopters" (AZ Industries)
Dr. Paul Brown, "Betavoltaic Batteries"
Dr. Deborah D. L. Chung, "Carbon Fiber Electrical Energy Generation" (SUNYAB)
Dr. Paulo Correa, "Excess Energy from Pulsed Abnormal Glow Discharge"
Dr. David Goodwin, "Summary of the Breakthrough Physics Conference" (DOE)
Dr. Peter Graneau, "Release of Chemical Bond Energy" (Center For EM Research)
James Griggs, "Hydrosonic Fluid Heater"
Bruce Perrault, "Radiant Energy Generator"
Chip Ransford, "Tabletop Nuclear Transmutation"
Kent Robertson, "American Wind Energy"
Dr. Ken Shoulders, "New Energetics"
Dr. Edmund Storms, "New Method for Initiating Nuclear Reactions" (Los Alamos Labs)
Dr. Tom Van Flandern, "Complete Gravity Model and Free Energy" (Meta Research)
David Wallman, "Carbon-Arc Gasification of Biomass Solutions" DW Research

Workshops

Dr. Paul Brown, "Effective Radioactive Waste Remediation"
Dr. Peter Graneau, "Experimental Results of Arc Discharge"
Michael Hackelman, "Home Electricity Power"
Dr. Gene Mallove, "Assisted Nuclear Reactions"
Dr. George Miley, "Low Energy Nuclear Reactions"
Paul Pantone, "GEET Clean Combustion Kit"
Chip Ransford, "Nucleosynthesis Free Energy Details"
Robert Rose, "Breakthrough Technology: Fuel Cells"
Ken Shoulders, "Charge Clusters in Motion Q & A"
Dr. Ed Storms, "Technical Details of the New Method"
David Wallman, "Biomass Solutions: Demonstration"

21st CENTURY
SCIENCE & TECHNOLOGY

Winter 1998-1999 Features:

Beyond Molecular Biology: The Biophoton Revolution
by Jonathan Tennenbaum

What Ever Happened to Cold Fusion?
by Edmund Storms

The Burn-wave Fission Reactor

The Decipherment and Discovery of a Voyage to America in 232 B.C.

The Population Implosion

Subscribe!
$25 for 6 issues (U.S.)
$50 for 6 issues (foreign)
Single copies $5 ($6 foreign)

Send check or money order (U.S. currency only) to
21st Century, P.O. Box 16285
Washington, D.C. 20041

Society for Scientific Exploration 18th Annual Meeting

June 3-5, 1999
University of New Mexico-Albuquerque

- Shaking the Pillars of the Paradigm
- Cold Fusion
- Reports on Anomalous Phenomena
- Suppression of New Science

For Registration Details:
http://www.jse.com/eighteenth.html

Seventh Russian Conference on Cold Nuclear Transmutation

May 23-30, 1999
Dagomys, Russia

Registration Fee: $900 (includes conference, meals, hotel, airport transportation). Contact: Yury N. Bazhutor, Co-Chair Organizing Committee, P.O. Box 169, "Erzion Center," 105077, Moscow, Russia

Ph: 095-464-78-81; Fax: 095-151-89-65
e-mail: prfnart@hotmail.com

Advertise Your Upcoming New Energy Event

Call *Infinite Energy* Magazine for details.
(603-228-4516)

33rd Intersociety Energy Conversion Engineering Conference (IECEC)
August 2-6, 1998
Colorado Springs, Colorado

Institute for New Energy (INE) Symposium
August 14-15, 1998
University of Utah
Salt Lake City, Utah

International Tesla Society Meeting
September 4-6, 1998
Colorado Springs, Colorado

Sixth Russian Conference on Cold Nuclear Transmutation
September 28-October 5, 1998
Dagomys, Russia

Infinite Energy Magazine's
Cold Fusion and New Energy Symposium 1998
October 11, 1998
Manchester, New Hampshire

Second International Symposium on Consciousness, New Medicine and New Energy
November 26-27, 1998
Waseda University
Tokyo, Japan

Conference on Free Energy U.S. State Department
April 29 - May 1, 1999
Washington, D.C.

The World Sustainable Energy Fair
May 25-27, 1999
Amsterdam RAI

Seventh Russian Conference on Cold Nuclear Transmutation
May 23-30, 1999
Dagomys, Russia

Society for Scientific Exploration 18th Annual Meeting
June 3-5, 1999
Albuquerque, New Mexico

ICCF-8, Eigth International Conference on Cold Fusion
Fall, 1999 in Italy
(Exact location pending)

New Energy Times Archive

FREE Gift if you visit OURPAL website
http://www.ourpal.com

OUR PAL(R) ASIJA
MBA, PE, CDP, JD, ATM
AFFORDABLE
Intellectual Property
Attorney

PH:203-924-9538, FAX:203-924-9956
E-Mail: Ourpal@ourpal.com
Jail-Mail: 203-924-4271
(AKA Voice-Mail)
Snail-Mail: 7 Woonsocket Avenue
Shelton, CT 06484
Free Inventrepreneurship Kit

OFFSHORE FINANCIAL EDUCATION
Asset Protection, Tax Minimization,
Privacy and Income Enhancement,
The International Marketplace,
Tapes, Books, Seminar,
Powerful, Unique, Legal,
Earn while you learn,
Phone Jim +1 (603) 763-5965 (EST) or
e-mail offshore@TakeLifeBack.com

Place your classified ad in *Infinite Energy* for international New Energy coverage.

$.50 per word

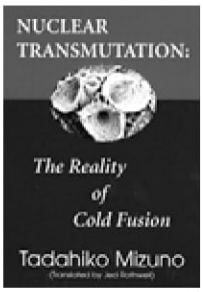
Cold Fusion Technology, Inc.
P.O. Box 2816, Concord, NH 03302
Phone: 603-228-4516; Fax: 603-224-5975
staff@infinite-energy.com

If you've been thinking about placing a classified ad – why not give us a try?

Available from Cold Fusion Technology, Inc.

BOOKS (ORDER FORM ON PAGE 128)

Prices reflect North America/Foreign costs (ex: $32.95/$39.95) and includes First Class/Airmail shipping.



Nuclear Transmutation: The Reality of Cold Fusion

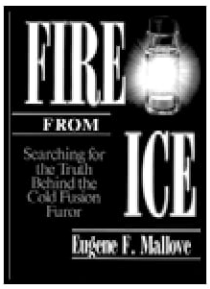
Tadahiko Mizuno
English Translation by Jed Rothwell

"This fascinating account by Prof. Tadahiko Mizuno of his personal experiences in cold fusion is 'must' reading for researchers in the field and for all others who have an interest in it, either 'pro' or 'con.' In the process of recounting his experiences and views, Dr. Mizuno provides an important glimpse into how research often evolves in practice and how the process can plunge into a chaotic maze." —George Miley
151 pp, Hardcover.....ITEM # B001, $32.95/$39.95

Fire From Ice: Searching for the Truth Behind the Cold Fusion Furor

Eugene F. Mallove

Fire From Ice chronicles one of the most misunderstood and valued scientific controversies of all time. It details the events surrounding and following the 1989 Pons and Fleischmann announcement of the discovery of cold nuclear fusion. Mallove offers a unique insider's view of the controversy, while at the same time explaining the relevant science and technology.
334 pp, Hardcover.....ITEM # B002, $25.95/$29.95



The Starflight Handbook: A Pioneer's Guide to Interstellar Travel

Eugene Mallove & Gregory Matloff

"*The Starflight Handbook* is an indispensable compendium of the many and varied methods for traversing the vast interstellar gulf —don't leave the solar system without it!" —Dr. Robert L. Forward
274 pp, Hardcover.....ITEM # B003, $34.95/$41.95

New Energy Times Archive

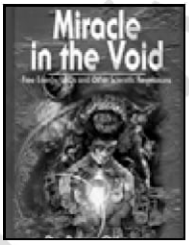
Prices reflect North America/Foreign costs (ex: $32.95/$39.95) and includes First Class/Airmail shipping.

Heretical Verities: Mathematical Themes in Physical Description

Thomas E. Phipps, Jr.

Phipps' dedicated study of the advancement of physics is clearly shown with his work, which critiques Establishment physics, publications and thinking. This book is a must read for those "concerned with the foundations of physics and mathematics who have not been blinkered or blinded by a narrow professional education."—H. Pierre Noyes

637pp, Hardcover.....ITEM # B004, $45.00/$60.00

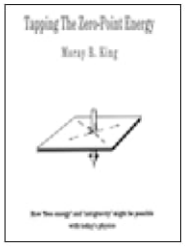


Miracle in the Void: Free Energy, UFOs and Other Scientific Revelations

Brian O'Leary

"Dr. O'Leary bluntly states that we can replace the current oil-based economy, renovate our nation-states and clean up the environment. *Miracle in the Void* is good reading for everyone who wants beneficial changes on the planet." —Jeane Manning

260 pp, Paperback.....ITEM # B005, $24.95/$29.95

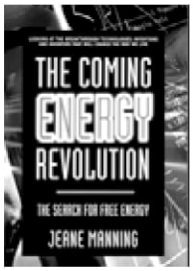


Tapping The Zero-Point Energy

Moray B. King

This book is a "must read" for the inventor. King explores how "free energy" and "antigravity" might be possible with today's physics.

173 pp, Paperback.....ITEM # B006, $12.95/$17.95

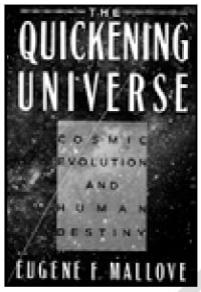


The Coming Energy Revolution: The Search for Free Energy

Jeane Manning

This 1996 book predicts that a new, society-changing energy revolution is coming. Manning examines inventions and the scientists behind them, and how they will change the world with "new energy."

230 pp, Paperback.....ITEM #B007, $15.95/$22.95

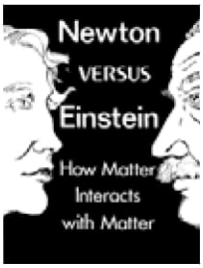


The Quickening Universe: Cosmic Evolution and Human Destiny

Eugene F. Mallove

"A wonderful philosophical and reflective essay on the nature of the universe as informed by modern science. It is a fine example of how scientific discovery helps shape one's world view in a rational yet compassionate manner." — Heinz Pagels, Ph.D.

268 pp, Hardcover..... ITEM # B008, $19.95/$24.95

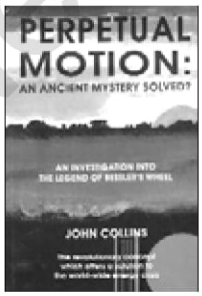


Newton Versus Einstein: How Matter Interacts with Matter

Peter Graneau & Neal Graneau

Newton Versus Einstein is not merely a critique of contemporary physics, nor just an outline of the debate between followers of the scientific philosophies of Newton and Einstein. It points the way to new technologies, from space accelerators to high-current switches and improved metal refining.

219 pp, Hardcover.....ITEM #B009, $19.95/$26.95



Perpetual Motion: An Ancient Mystery Solved?

John Collins

Johann Bessler claimed in 1712 that he had discovered the secret of perpetual motion. Startling new evidence recently found in unpublished documents seems to vindicate his claims. Painstaking research into these claims and a study of the precise tests carried out add dramatic new details which prove beyond a doubt that a form of energy generation, which appears to be perpetual motion, is possible. Could this three-hundred-year-old technology lead to a solution to the energy crisis?

240 pp, Paperback.....ITEM #B010, $25.00/$32.00

New Energy Times Archive

Prices reflect North America/Foreign costs (ex: $32.95/$39.95) and includes First Class/Airmail shipping.

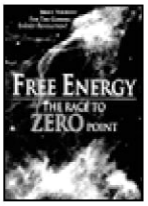
God's Secret Formula: Deciphering the Riddle of the Universe and the Prime Number Code

Peter Plichta

"This book is fascinatingly written. The content is mathematically flawless. For the first time in the history of humanity, the scientific studies of Dr. Plichta provide a foundation for our physical world view. Everything that preceded Plichta - from Newton to Einstein - was only theory." — Professor Rupert Lay

218 pp, Hardcover.....ITEM #B011, $29.95/$39.95

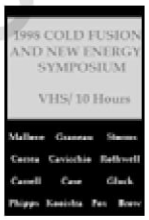
VIDEOS



Free Energy: The Race to Zero Point

This comprehensive 1997 documentary features the most promising devices, processes and theories of the most visionary scientists and the most persistent independent inventors on the planet.

VHS, 110 minutes.....ITEM # V001, $39.95/$49.95



1998 Cold Fusion and New Energy Symposium

October 11, 1998. Ten hours of outstanding New Energy lectures are available on videotape. Lectures by P. Graneau, E. Storms, P. Correa, D. Cavicchio, J. Rothwell, M. Carrell, L. Case, P. Glück, T. Phipps, J. Kooistra, H. Fox, P. Brown and more.

10 Hours, 5 VHS tapes.....ITEM # V002, $150.00/$175.00

1996 Cold Fusion/New Energy Symposium

January 20, 1996. E. Mallove, J. Rothwell, M. Swartz, D. Cravens, J. Patterson, J. Griggs, H. Fox, K. Johnson, C. Tinsley, P. LaViolette, H. Puthoff, Japanese Magnetic Motor Researchers; includes views of the Power-Gen 95 CETI Patterson Power Cell demo equipment, magnetic motors, video clips of the "Cincinnati Group's" experiments, operation of the Hydrosonic Pump™, video footage of Yusmar devices in Moldova, etc.

7 hours, 4 VHS tapes.....ITEM # V003, $99.95/$124.95

MIT Cold Fusion Day 1995

January 21, 1995. E. Mallove, P. Graneau, F. Jaegar, P. Hagelstein, K. Johnson, J. Griggs and more. . . Includes footage of the hydrosonic pump and other devices.

8 hours, 4 VHS tapes.....ITEM # V004, $84.95/$109.95

Fifth International CF Conference

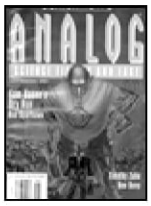
April 9-13, 1995 (Monaco). M. McKubre, S. Pons and M. Fleischmann, F. Celani, D. Cravens, M. Srinivasan, P. Hagelstein, Y. Arata, H. Fox, T. Passell, and many more. Eight hours of presentations! Included extra: all abstracts and an index of participants. 4 VHS tapes.....ITEM # V005, $200.00/$230.00

Also Available:Video Tapes of the Third and Fourth International Symposiums on New Energy in Denver, Colorado. Over 70 individual tapes available, each containing a lecture by a given speaker. $20.00 Domestic and Foreign (Postage: US $3.00 each first tape, $1.00 each additional; Foreign: $6.00 each first tape, $5.00 each additional). Call or write for a complete listing.

Miscellaneous Products

WIRED Magazine

Charles Platt's eighteen page exposé feature article in the November 1998 *Wired*, "Dirty Science: The Strange Rebirth of Cold Fusion," makes it abundantly clear that the world very likely is facing an energy and scientific revolution of unprecedented scale. ITEM #M001, $12.00/$ 17.00



Analog Magazine

July/August 1997. Twenty-one page article by Dr. Eugene Mallove, "Cold Fusion: The 'Miracle' is No Mistake." Inscribed by Dr. Mallove. ITEM #M002, $10.00/$15.00

Popular Science Magazine

August 1993. Six page article by Jerry Bishop, "It Ain't Over Till It's Over. . . Cold Fusion." Inscribed by Dr. Mallove. ITEM #M003, $10.00/$15.00

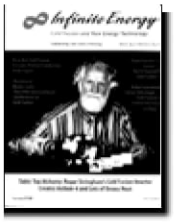
Zero Point Technologies CD

Frank Znidarsic. Runs on any computer with a browser (Mac 8.0, Win 95/98). Contains: theory, reports, photos, movies, sound bites and index of various zero point experiments. ITEM # M004, $19.95/$23.95

New Energy Times Archive

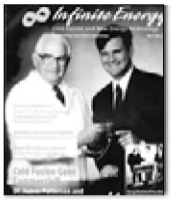
Back Issues of *Infinite Energy* Magazine (see page 128 for price information)

Volume 1—1995-1996



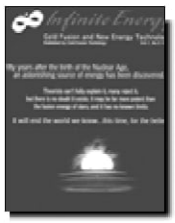
Issue No. 1 (52 pages)

• Cold Fusion: A Brief History of Mine (Nobel Laureate Julian Schwinger) • Welcome to the Apocalypse? (Arthur C. Clarke) • Very Hot Cold Fusion: Mizuno's Ceramic Proton Conductors • Cold Fusion: From Reasons to Doubt to Reasons to Believe • Table-Top Alchemy: Roger Stringham's Ultrasonic Cold Fusion Creates Helium-4 and Helium-3 and Lots of Excess Heat • Testing the Patterson Power Cell • Water Cavitation Reactor • Japan TV Features "Over-Unity" Magnetic Motors • Micro-Fusion Reactor



Issue No. 2 (56 pages)

• Cold Fusion Goes Commercial: Clean Energy Technologies, Inc. (CETI) Aims to Help Clean Up the World • Fifth International Conference on Cold Fusion • Alchemy Nightmare: Skeptic Finds Heavy Element Transmutation in Cold Fusion Experiment • Magnetic Resonance Amplifier • Flowing Electrolyte Calorimetry • Moldova Water Fuel Device • Cold Fusion in 2001 and Beyond• Unitary Quantum Mechanics



Issue No. 3 (60 pages)

• Low Energy Transmutation Conference • Testing Excess Heat Cavitation Devices • Cold Fusion Companies and Products • Applying the Nucleon Cluster Model to Experimental Results • Table-top Sonoluminescence • How to Miss the Energy Boat • Book Review: *A Dialogue on Chemically-Induced Nuclear Effects* • Mechanisms of a Disobedient Science: A Cold Fusion Theory, Part I

Issue No. 4 (60 pages)

• Pons and Fleischmann Patent to Issue in Europe • Mizuno's Solid-State High Temperature Cold Fusion • 80-to-1 Cold Fusion Demo at Hot Fusion Meeting • Atomic Bomb Physicist (Dr. Joseph McKibben) Proposes Startling Cold Fusion Theory • The Piantelli Cold Fusion Patent • ICCF5 Talk by Los Alamos' Dr. Edmund Storms • Kawai Magnetic Motor • Acoustic Levitation • The New Scientific Age • Cold Fusion Documentary, "Too Close to the Sun," on BBC and Canadian TV

Double Issue No. 5/6 (120 pages)

• CETI's 1-kilowatt Cold Fusion Demo • *Infinite Energy's* 1996 Cold Fusion Symposium • Patterson Power Cell on ABC-TV "Good Morning America" and "Nightline" • Latest Patterson Patent • Takahashi's Super Magnets • Master Principle of EM Over-Unity • HydroCatalysis Power Corporation • Chinese and Russian Cold Fusion Conferences • Transmutations in Palladium • The Nucleon Cluster Model • The Energy Active State in Pd-D • Mechanisms of a Disobedient Science, Part II • Travels in the New Energy Age • Zero-Point Energy • **COLD FUSION RESOURCE GUIDE —1996** (Frequently Asked Questions; Cold Fusion, What it Does; Basic Information Resources; Seminal Articles: Experimental Evidence; Highlights of Cold Fusion History: A Timeline)

Volume 2—1996-1997

Issue No. 7 (72 pages)

• The Correa Pulsed Abnormal Glow Discharge Reactor •Japan Research Yields Transmutations in Metals • International Symposium on New Energy-III • Los Alamos Lab's Cold Tritium Generator Goes Public • NASA Confirms Excess Energy • Cold Fusion and New Energy—An Environmentalist's Perspective • Book Reviews: *The Energy Machine of Joseph Newman*, *Longitude* and *Revolution in Time*

Issue No. 8 (64 pages)

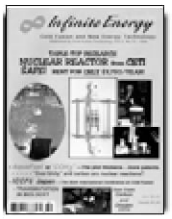
•Overview of the Correa Invention • The Reality of Perpetual Motion • A "Chain Reaction" of New Energy Movies (*Chain Reaction* Debuts) • Properties of Palladium and the PF Effect • Interviews: Dr. John Bockris and Dr. Hal Puthoff • Super Tube • Controlled Transmutation of Elements • Ultrasonic Momentum Transfer Pump

New Energy Times Archive

Back Issues of *Infinite Energy* Magazine

Issue No. 9 (72 pages)

• Prof. George Miley Confirms Low Energy Transmutations in Patterson Cell • "SuperPower, Inc." Investment Fund • Landmark Low Energy Transmutation Conference in Texas • AquaFuel: Wonder Fuel of the New Energy Age • The Wright Brothers and Cold Fusion • "Anti-Gravity" Experiments Go Public • Mass Modification Experiment Definition Study • Book Review: *The Coming Energy Revolution* •Correa PAGD Reactor



Issue No. 10 (72 pages)

• CETI's Table-Top Research Nuclear Reactor • Sixth International Conference on Cold Fusion • Transmutation in Biology? • Experimenter's Corner: Iron from Carbon, Oxygen, and Hydrogen? • "Over-Unity" in Carbon Arc Nuclear Reactions • Adams-Aspden Motor Patent •Ragland Triode Cold Fusion Cell • Open Letter to Japan's NHE Cold Fusion Lab Directorate •Breakthrough in Thermoelectrics • AquaFuel

Issue No. 11 (80 pages)

• Interview: Dr. Martin Fleischmann • *The Saint* Cold Fusion Movie • Vacuum Energy Patent Issued • Zero Point Energy Confirmed at Los Alamos • More AquaFuel Testing • Review of EPRI -SRI International Cold Fusion Report • Coping with the U.S. Patent Office on Cold Fusion • BlackLight Power Corporation • Book Review: *Newtonian Electrodynamics* by Peter and Neal Graneau • Experimental Evidence for Nucleon Cluster Model • Adams-Aspden Motor Patent

Issue No. 12 (72 pages)

• BlackLight Power Corporation and Randell Mills • Arthur C. Clarke on Cold Fusion • Cold Fusion and the Future, Part I • Cold Fusion Cell Makes DOE Cold Fusion Critic Blink • Arata and Zhang: Massive Helium/Heat Production in Cold Fusion • Protonic Conductors and Cold Fusion — A Russian Perspective • Charge Clusters in Operation • Book Reviews: *Wizard : The Life and Times of Nikola Tesla* and *Gravitational Force of the Sun* • Energy Gain in Low Energy p and D Nuclear Reactions • Palladium, Platinum, Gold Futures • Drs. Patterson and Cravens Receive New Cold Fusion Patent • CETI/ENECO Patent Dispute in Europe • Interview: Dr. Robert T. Bush

Volume 3—1997-1998

Special Double Issue No. 13/14 (128 pages)

• Remediation of Nuclear Waste—Demo Transmutation Units on Sale! • New Energy Investment Fund Launched • Cold Fusion and the Future, Part II • Tesla Symposium • Extracting Intermolecular Bond Energy from Water • Carl Sagan and Cold Fusion • Science Writers and Cold Fusion • Whither *Hot* Fusion? • Paul Brown and Nuclear Battery • Continuing Research into T.H. Moray • The Robert Adams Story • Neutrino Power? • Adams Thermo-Motor Generator • Testing Ragland Triode Cell • Book Review: *Yes, We Have No Neutrons* • Interview: Martin Fleischmann • Interview: Eugene Mallove and T. Kenneth Fowler

Special Double Issue No. 15/16 (128 pages)

• Dr. Melvin Miles' *et al.* US Navy Report Nails Down Evidence for Cold Fusion—Excess Heat and Helium• Radioactivity Reduction Patent Issued to CETI • Lent-1 Transmutation Reactor • Historical Reports on Claims of Low-Energy Transmutation • UV Excimer Laser Production of New Elements on Cold Fusion Metals • What Nobel Laureate Glenn Seaborg Told President Bush about Cold Fusion Only Three Weeks after the Utah Announcement • "Gold Fusion:" The Economics of Alchemy • Can the Vacuum be Engineered for Spaceflight? • Modern Alchemy: Historical Reports

Issue No. 17 (96 pages)

• Cold Fusion and Transmutation Conference in Italy • Interview: Dr. Randell Mills of BlackLight Power, Inc. • Mills Patent Granted in Australia • Marinov Motor Proven to Violate Standard EM Theory—Repeatable, Simple Experiments • More on the PAGD™ Reactor Excess Electrical Energy Phenomenon • Three New Reports of Early 20th Century Low-Energy Transmutation—Papers in Mainstream Science Journals! • Book Reviews: *Heretical Verities* and *Nuclear Transmutation, The Reality of Cold Fusion* • Alchemy: Historical Reports • Cincinnati Group Transmutation—More Evidence Emerges • New Feature: Cold Fusion Comix

New Energy Times Archive

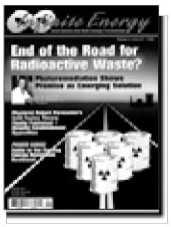
Back Issues of *Infinite Energy* Magazine

Issue No. 18 (88 pages)
• Nuclear Augmented Combustion Emerges • Review of Arata-Zhang's Cold Fusion: Helium and Excess Heat • US Army Report on Biological Transmutation • New Feature: "Words to Eat:" Silly Sayings of the Cold Fusion Skeptics • Enter "Miracle #2": Electrochemical Activation Method for Cheap, Rapid Water Sterilization • Testing of Adams-Aspden Motors • The President and the "HeavyWatergate" Scandal • Dieter Britz • Ken Shoulders' EV Technology Reviewed • The Marinov Motor is NOT a Homopolar Generator • Conventional Electric Motors • Alchemy: Historical Reports

Volume 4—1998-1999

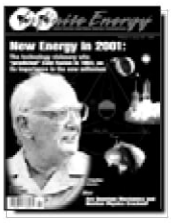
Issue No. 19 (88 pages)
• Kinetic Furnace Enters New Energy Race: High Over-Unity Cavitation Commercial Device • Dr. Les. Case's Catalytic Fusion Process: Helium and Excess Heat from Standard Catalysts • Seventh International Conference on Cold Fusion • Marinov Motor • Aspden's Cold Fusion-Related Patent • *Analog* Editor Blasts Status Quo • AquaFuel • Cavitation in D_2O with Metal Targets Produces Predictable Excess Heat • Update on Nuclear Augmented Combustion

Issue No. 20 (88 pages)
•Do-It-Yourself CF Experiment: Excess Energy Evolution and Transmutation/Ohmori-Mizuno •American Nuclear Society Annual Meeting• ICCF-7 Continuing Coverage/Abstracts • Arthur Clarke Speaks Out on Cold Fusion • Society for Scientific Exploration Meeting • Yull Brown's Death • Book Review: *Nuclear Transmutation : The Reality of Cold Fusion* • Rhodium-Catalyzed Fusion in Palladium • Theory of Excess Energy in a PAGD Reactor • Society for Scientific Exploration Meeting • Detection of Anomalous Elements (Mitsubishi) • Cold Fusion Meeting in Italy



Issue No. 21 (72 pages)
• Neutralizing Nuclear Waste Using Applied Physics • Cold Fusion Revisited • Hans Coler's Free Energy Generators • Cold Fusion in Deuterated Metals • Book Reviews: *Perpetual Motion* and *Power Surge* • Institute of New Energy Symposium • "Negative Resistance" Discovered? • Why Free Energy is Mathematically Possible

Issue No. 22 (72 pages)
Arthur C. Clarke: The Man Who Predicted Cold Fusion and Modern Alchemy (profile and book review) • Arthur C. Clarke's "2001: The Coming Age of Hydrogen Power" • Cold Fusion and New Energy Symposium • Aqueous Arc Experiment • The Quirks and Quarks of Physics and Physicists • *Wired* Magazine Features Cold Fusion • Plasma Electrolysis • Hadronic Mechanics Theory, Part I • Natural Convection Heat Transfer • Kinetobaric Effect for New Propulsion Principle? • Zinsser-Effect Device • Force and Gravity • Zen and the Art of Debunkery

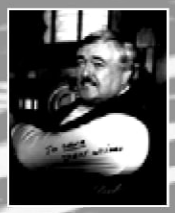


Issue No. 23 (80 pages)
Progress in Catalytic Fusion • Kinetic Furnace Update • Joseph Newman's Energy Machine • Cold Fusion: Historical Perspectives • Investing in New Energy Technologies • Hadronic Mechanics Theory, Part II • Proton-Electron Fusion Experiment • Perpetual Motion Theory from 1829 • Non-Stellar Nucleosynthesis • ***INFINITE ENERGY* INDEX**, Issues No. 1-23

Forthcoming in April 1999—The Ultimate Cold Fusion Video Documentary!

Cold Fusion: Fire from Water

Narrated by actor James Doohan ("Scotty" from Star Trek)



Organized and written by Dr. Eugene Mallove and Jed Rothwell with Video Production by Free Spirit Productions. Features the history, science, technology, and future of cold fusion. Interviews with numerous cold fusion scientists. Visits to laboratories and conferences. Appearance by Arthur C. Clarke.

This is a longer version (60+ minutes) of a documentary program that will air on select cable TV channels later in 1999. VHS (NTSC format) tapes expected by the end of April 1999. It will be available in VHS (NTSC format) or PAL format. ($34.95 North America/$44.95 Foreign)

New Energy Times Archive

Infinite Energy Magazine and Product Order Form

Name:_____
Address:_____
Address:_____
City:_____State:_____ Postal Code/Zip:_____
Country:_____ Phone:_____ Fax:_____

If using Credit Card: Check one: Master Card ____ VISA____American Express____
Card Number: _____Expiration Date: _____
Signature_____ E-Mail address_____

Infinite Energy Magazine (ISSN 1081-6372)

Subscriptions are $29.95 North America/$49.95 Foreign
Any six single back issues $29.95/$49.95
Single Issues $5.95/$10.00; Double Issues $10.00/$15.00
Prices in catalog reflect North America/Foreign costs (ex: $32.95/$39.95).
Price includes First Class/Airmail shipping.

TO ORDER BOOKS AND OTHER ITEMS OR TO SUBSCRIBE OR RENEW INFINITE ENERGY MAGAZINE INDICATE INFORMATION BELOW.
(SHIPPING AND HANDLING INCLUDED IN PRICES)

QUANTITY	ITEM #	PRODUCT	PRICE NORTH AMERICA	PRICE FOREIGN
		NEW Subscription — Start at Issue No.		
		RENEW Subscription — Start at Issue No.		
		Single Back Issue(s) No.		
		Double Back Issue(s) No.		
			TOTAL	

Cold Fusion Technology, Inc. P.O. Box 2816, Concord, NH 03302-2816
Fax: 603-224-5975; Phone: 603-228-4516;
e-mail: staff@infinite-energy.com
website: http://www.infinite-energy.com

Now going into our fifth year, *Infinite Energy Magazine* has been expanding distribution on U.S. newsstands. Typical 1998/1999 press run: 5,000 - 10,000 copies/issue. It is an international technical magazine with outreach to the general public as well. To maintain the highest editorial standards, it is written and edited by scientists, engineers, and expert journalists. It is aimed at pioneering scientists, engineers, industrialists, and investors who are entering an exciting R&D area. *Infinite Energy* now has subscribers in thirty-eight countries!

This area continues to grow explosively, with significant involvement by corporations and institutions in the U.S., Japan, France, Italy, India, Russia, and China. New technology developments and scientific discoveries are being made monthly. ***Infinite Energy*** reports on the latest information that is now pouring in from research centers around the globe.

New Energy Times Archive